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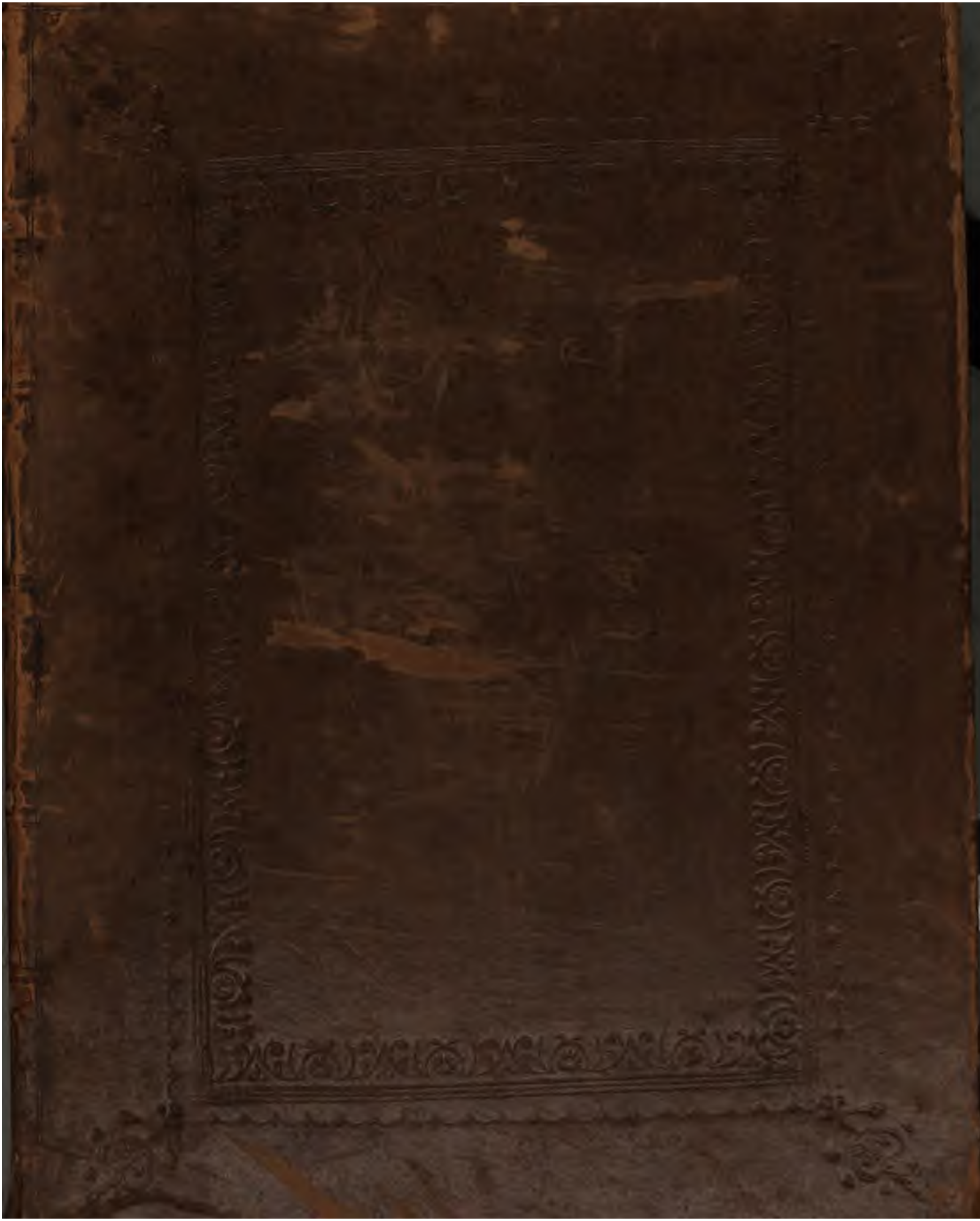

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PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Preſent Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS

IN MANY

Conſiderable Parts of the WORLD.

VOL. LII. PART I. For the Year 1761.

LONDON:
Printed for L. DAVIS and C. REYMERS,
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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philoſophical Tranſactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in ſeveral former *Tranſactions*, that the printing of them was always, from time to time, the ſingle act of the reſpective Secretaries, till the Forty-ſeventh Volume. And this information was thought the more neceſſary, not only as it has been the common opinion, that they were publiſhed by the authority, and under the direction, of the Society itſelf; but alſo, becauſe ſeveral authors, both at home and abroad, have in their writings called them the *Tranſactions of the Royal Society*. Whereas in truth the Society, as a body, never did intereſt themſelves any further in their publication, than by occaſionally recommending the revival of them to ſome of their ſecretaries, when, from the particular circumſtances of their affairs, the *Tranſactions* had happened for any length of time to be intermitted. And this ſeems principally to have been done with a view to ſatisfy the public, that their uſual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their firſt inſtitution by the Royal Charters, and which they have ever ſince ſteadily purſued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviſeable, that a Committee of their Members ſhould be appointed to reconſider the papers read before them, and ſelect out of them ſuch, as they ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26 of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of nature or art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons, through whoſe hands they receive them, are to be conſidered in no other light, than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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PHILO-

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of the Uſe of Furze in fencing the Banks of Rivers: In a Letter to the Reverend* Stephen Hales, *D. D. F. R. S. from the Reverend Mr.* David Wark.

Reverend Sir,

Read Jan. 8, 1761.

I HAD occaſion to inform you before, that on obſerving a little ſand placed in the midſt of a river, where the ſtream was pretty rapid, I inquired into the cauſe, and found a furze buſh lodged there, which had detained the ſand, in ſpite of the current. It was eaſily concluded from hence, that furze might be profitably uſed in fencing the banks of rivers, at a very cheap rate, and thereby preventing many acres of rich ſoil from being changed into barren gravel. Several years after, I prevailed on ſome gentlemen of my acquaintance to try the experiment; which was ſo cheaply done, and followed with ſuch remarkable ſucceſs, that numbers ſoon followed their example: ſo that it is now almoſt univerſally practiſed here; and, hitherto, has never been once known to fail, in anſwering the deſign. In purſuing the ſcheme, I found, upon trial, that locks and damheads might be raiſed, at one tenth of the ordinary expence, by the help of furze, as a very thin perpendicular wall of ſtone and lime, or one of deal-boards, two inches thick, is the principal part of the expence. Cloſe to this wall, on the other ſide, is a mound of furze intermixed with gravel, and along the top of it (of the wall, viz.) a ſtrong tree, equal with the higheſt part of the mound. It is plain, this wall cannot be hurt by the weight of the water, or force of the current, as it is defended by the contiguous mound, which is ſix or ſeven yards broad; nor can the preſſure of the mud and gravel make it give way, as their weight is ſuſpended by the interweavings of the furze. If, therefore, the tree on the top of the wall can be made to keep its place, the whole is firm.

It is well known, that they make their ſea-dykes in Holland with faggots of any ſort of bruſh-wood; and it muſt appear to any one, who examines the net-work formed by the croſſings of the branches and prickles of furze, that it is far more effectual for this purpoſe, both as it detains the collected earth, and is far more cheaply procured than faggots.

I hope it will be eaſily obſerved, from what has been ſaid of locks and damheads, that a great deal of expenſive ſtone-work in building harbours may be avoided, by the help of furze mounds.

I am,
With the greateſt reſpect,
Reverend Sir,
Your moſt obedient,
humble ſervant,

Hadn. Dec. 13, 1760. Da. Wark.

II. *An Account of a remarkable Halo: In a Letter to the Rev.* William Stukeley, *M. D. F. R. S. from* Tho. Barker, *Eſq*;

Reverend Sir,

Read Jan. 8, 1761.

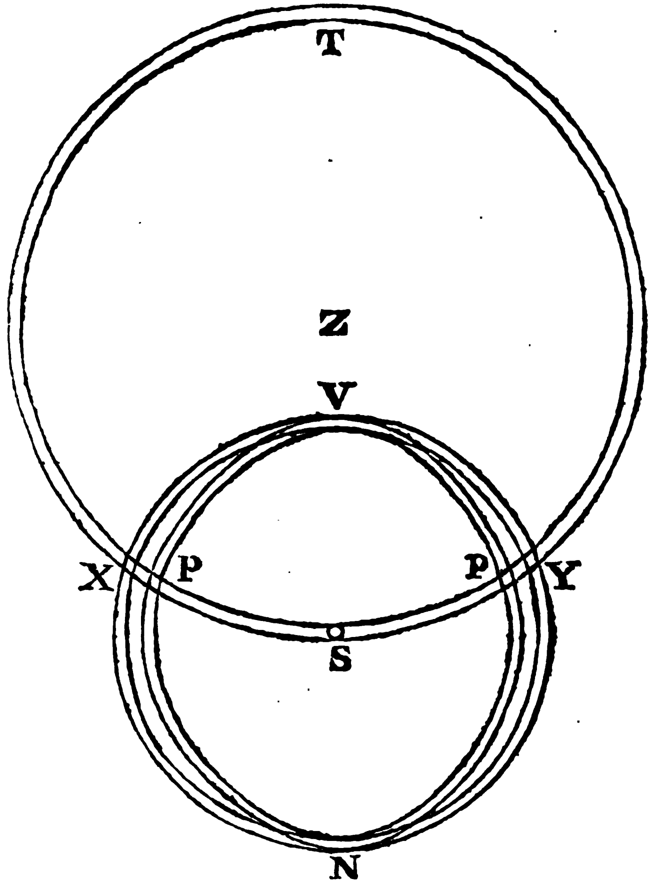
I Thank you for preſenting my paper on the Dog ſtar to the Royal Society; the opinion advanced in which is ſo very unuſual, that I expect it will be at once rejected, as incredible, by all, who do not care for the work of examining the evidence for it. But I ſhould be glad to hear, that ſome impartial perſon had carefully ſearched, whether what I have ſaid be ſupported by fact, and what other evidence can be found, which I have miſſed, either in ſupport or confutation of that change of colour in Sirius, which I have ſuppoſed.

I have long neglected to acknowlege the favour of your information about the comet in Orion laſt January; but had nothing particular to ſay about it, not having the luck to ſee it. I did not happen to look out on the Tueſday night, when it was ſeen; ſo heard nothing of it, till the news-paper on Saturday, when I did look for it with my naked eye and teleſcope alſo; but as it was dwindled, I did not find it; and the rather, as its motion was ſo ſwift, I could not, ſo many nights after, know well where to look for it. The comet of 1664, might have appeared nearly in the ſame place this was ſeen, with a ſwift motion, a pretty many degrees in a day, as a retrogade comet in oppoſition to the ſun generally has; but, I think, would not have been near enough to have moved a degree in an hour, as this did; and I think it would alſo have been larger, and continued longer, than this; for in 1664, it was ſeen four months, and when far diſtant from the earth; and, in the poſition it muſt have been in laſt January, would hardly have gone farther back than the beginning of Gemini, in ſmall N. latitude, and is, I believe, one of the largeſt comets.

I have long had by me an account of a remarkable halo, I was called out to obſerve, May 20, 1737, a quarter before eleven in the morning, and which continued half an hour, in a clear hot ſky; and was as in the figure.

The common halo V X N Y, and the horizontal white circle S X T Y, were no way different from uſual; nor were any parhelia ſeen. All, that was remarkable, was an elliptical halo V P N P, coinciding at the top and bottom with the common one, but four degrees narrower in the leſſer diameter at P and P, coloured juſt like the halo, and at the coinciding places, eſpecially at V, very bright.

I call



I call VPNP the elliptical halo, becauſe it appeared ſo to me; yet, as the horizontal diameter was only gueſſed at, and nothing meaſured, but the altitudes of the points S, V, N, and T, which gave the diameter of the halo VN 45°, I will not be poſitive, that VPNP was not the circular one, and VXNY elliptical, and 4° wider than the circle at X and Y. Which ever it was, it is, I think, worth preſerving, as I do not know we have any account of ſuch another, unleſs what Dr. Halley, in Philoſophical Tranſactions, N° 278. calls two arches of circles touching the halo at top and bottom, can be ſuppoſed to be imperfect parts of an elliptic halo not wholly ſeen.

With all due reſpect, I remain,

SIR,

Your humble ſervant,

Lyndon, March 3, 1760. T. Barker.

III. *An Account of a Meteor ſeen in* New England, *and of a Whirlwind felt in that Country: In a Letter to the Rev.* Tho. Birch, *D. D. Secretary to the Royal Society, from Mr.* John Winthrop, *Profeſſor of Philoſophy at* Cambridge *in* New England.

Reverend Sir,

Read Jan. 15, 1761. I Am extremely obliged to the Royal Society, for their favourable acceptance of my paper on our late great earthquake; and to you, Sir, for the very polite manner, in which you were pleaſed to inform me thereof. I wiſh I were able to communicate any thing worthy the attention of ſo illuſtrious a body. But no ſuch thing occurs at preſent; unleſs you ſhould be of opinion, that the two following accounts, in the meteorologic way, are ſo in ſome degree.

The firſt is of a meteor, by which the ſouthern parts of this province were greatly alarmed, on Thurſday, the 10th of May laſt, about 35′ after Nine in the morning. The weather being then fair and calm, the people at Bridgewater, and the towns near it, about 25 miles ſouth from hence, were ſurprized with a noiſe, like the report of a cannon, or volley of ſmall arms, which ſeemed to come from the weſt. This report was followed by a rumbling noiſe, which moſt took for the roar of an earthquake; and, when it had laſted about a minute, there was another exploſion, like that of a cannon; and about as long after, a third; the roaring noiſe, in the mean time, increaſing, ſo as to fill the air all around, to the great terror and amazement of thoſe who had heard it, as ſome of them have informed me. After this third exploſion, the noiſe gradually abated, ſeeming to go off toward the ſouth-eaſt; having laſted, in the whole, as was judged, about 5′. This is all I can collect of the real fact, from the ſeveral accounts given in thoſe places, where the noiſe was loudeſt. That ſound, which moſt took for the roar of an earthquake, ſome compared to the beating of drums; and added ſome circumſtances, with relation to it, too whimſical to be here repeated. It is ſufficient to obſerve, in general, that they were ſuch as were probably ſuggeſted by an imagination prepoſſeſſed with ideas of war, and, at that time, terrified to a great degree.

As to the extent of theſe noiſes, they were heard as far north as Roxbury and Boſton; eaſt, a league beyond Cape Cod; ſouth, at Martha's Vineyard and Rhode Iſland; and weſt, at Providence and Mendon; filling a circle of about 80 miles in diameter, the center of which was at Bridgewater, or near it.

The meteor, which produced theſe noiſes, was not ſeen near the center of this circle, but only near the circumference. The moſt diſtinct account I have had of it, was from a creditable perſon at Roxbury, a town adjoining on Boſton, who informed me, that, about ten o'clock that morning, he ſaw in the air a ball of fire, about 4 or 5 inches in diameter, drawing a train of light after it. The ball was of a white brightneſs, exceeding, in his opinion, that of the ſun. Though the ſun then ſhone out clear, this fire-ball was bright enough to caſt a ſhade, by which he firſt perceived it in the ſouth-eaſt, paſſing below the ſun. For he was ſtanding with his back toward that and the ſun; but this ſhade put him upon turning round, to diſcover what might be the cauſe of it. He ſays, the ball moved parallel to the horizon from the north-eaſt toward the ſouth-weſt, not above half ſo faſt as ſhooting ſtars generally do, and diſappeared while he was looking on it; and that about 4 or 5′ after, he heard a kind of rumbling noiſe, ſomewhat like that of an earthquake; which was alſo heard by many others in Roxbury.

From a veſſel about a league ſouth-weſt from Cape Cod, and from Martha's Vineyard, we have received like accounts of a bright ball in the heavens, ſufficient to aſcertain the reality of the meteor, but not to determine its hight and courſe. Near the center of the fore-mentioned circle, the meteor muſt have paſſed too near the ſun to be viſible.

The other account I had in view, is of a whirlwind, which happened on Tueſday, the 10th of this inſtant July, at Leiceſter, a town in this province, ſituated about 40 miles weſt from hence. In point of violence, it ſeems to have equalled any, and exceeded moſt, that have happened in this country, ſo far as I can judge by the accounts I have ſeen, and, indeed, moſt, that are recorded in the Philoſophical Tranſactions. I was very deſirous to have gone myſelf, to take a view of its deſtructive effects, but an infirm ſtate of health has prevented me. However, I have received ſuch informations from ſeveral gentlemen, who have been on the ſpot, as enable me to give a particular account of it; in which I ſhall relate nothing but what, I am well aſſured, may be relied on as fact.

The morning of the 10th July with us, at Cambridge, was fair and hot, with a briſk gale at ſouth-weſt. The afternoon was cloudy. About five, it began to rain, and thundered once. At Leiceſter, ſeveral people of credit ſay, that about five o'clock the ſky looked ſtrangely; that clouds from the ſouth-weſt and north-weſt ſeemed to ruſh together very ſwiftly, and, immediately upon their meeting, commenced a circular motion; preſently after which, a terrible noiſe was heard. The whirlwind marched along from ſouth-weſt to north-weſt. Its firſt effects were diſcernible on a hill, where ſeveral trees were thrown down, at conſiderable diſtances from each other. On the north-eaſt ſide of this hill, was a tree, which ſeemed to have been ſtript of its limbs on the ſouth ſide, nearly from the top to the bottom. At the foot of the hill was a ſwamp, through which

the

the progreſs of the wind could not be followed, without great difficulty; though, by the appearance of the ſwamp from the hill, the violence ſeems to have been increaſed. After paſſing the ſwamp, it ſtruck the open ſide of a hill with prodigious force. Here lay a great number of large ſtones, many of which were thrown out of the beds they had made; particularly one, judged to be near 150 ℔, was moved from its place 3 or 4 feet; and others, which were ſmaller, to greater diſtances. Here alſo lay the trunk of a great tree, 2½ feet in diameter at the butt-end, and about 40 feet long, which was rolled over, one turn, out of its bed, toward the upper part of the hill. The trees on the ſide of this hill, and in a valley to the ſouth of it, did not ſtand thick, but were, in general, large: moſt of theſe were torn up by the roots, and thrown down in almoſt all directions; many at right angles to the courſe of the wind, ſome with their tops ſouth-eaſt, others north-eaſt; one, which had been broken off about 10 feet from the ground, lay with its top about ſouth-weſt, that is, contrary to the courſe of the wind. The current of air at that place was judged to have been about 40 rods wide, from the ſide of the hill acroſs the valley before-mentioned; its greateſt violence being, by its effects, diſcernible along the ſide of the hill. Having then paſſed over ſome clear land, for about half a mile, on which it left no other marks than part of a corn-field levelled, and the ſtone-walls and fences thrown down, it came to the dwelling-houſe of one David Lynde, the only one, which ſtood in its way: upon this it fell with the utmoſt fury, and, in a moment, effected its complete deſtruction,

ſtruction, as I ſhall preſently relate. About 3 or 4 rods before it came to the houſe, it took up an apple-tree by the roots, and carried it into the yard before the houſe. After paſſing the houſe, and throwing down the fences, and ſeveral trees, which ſtood in its courſe, it ſeemed, by the effects, to have altered its direction a little more to the eaſtward. In this direction, it paſſed through a field of grain, in which it made a lane of 8 or 10 rods wide; from whence it proceeded through a ſwamp, where, by a view from the ſide of it, it appeared to have made great havock; and after this, it paſſed over a pond about half a mile diſtant from the houſe. No effects of it were viſible upon the ground to a greater diſtance than 4 miles from the houſe, north-eaſtward, or about 6 miles from the place where it began.

To come now to the deſtruction of the houſe. This was in the form of an ┘; one part fronting the ſouth, on the country road, from which it ſtood back about 2 rods; the other part fronting the eaſt. In the middle of the ſouth front was a door, diſtant from the chimney about 4 feet. Behind the eaſtern room was the kitchen, the chimney of which ſtood at the north end; and the door of it was in the eaſtern front. The houſe was of wood, two ſtories high; and both the chimneys of ſtone, Near the houſe were a ſhop and ſmall ſhed; and the barn ſtood on the oppoſite ſide of the road, ſouth, about 10 rods diſtant. As ſoon as they perceived the ſtorm coming near the houſe, ſome men within endeavoured to ſhut the ſouth door; but before they could effect it, they were ſurprized by the falling of ſtones around them, from the top of that chimney, which was in the middle of the houſe. All the people in the houſe were, in that inſtant, thrown into ſuch a conſternation, that they can give no account of what paſſed during this ſcene of confuſion, which was, indeed, very ſhort. It may be judged of by the effects, and by the teſtimony of credible men, who lived near, and, in a few minutes after the wind, viewed the deſolation made by it. Where the houſe ſtood, nothing remained but the ſills, and the greater part of the lower floor, with part of the two ſtacks of chimneys, one about 10 feet, and the other not quite ſo high; the ſtones, which had compoſed the upper part, lying all around them; and the ſills, at the ſouth-weſt corner, were ſtarted out of their places round to the northward. Except theſe ſills, there were only three pieces of timber, and thoſe very large, left intire; one of which, about 16 feet long, and 10 inches by 8, was found on the oppoſite ſide of the road, nearly ſouth, about 20 rods diſtant from the houſe. The reſt of the timbers, from the greateſt to the leaſt, lay broken and twiſted to pieces between N. N. E. and E. for 70 or 80 rods from the houſe; ſome upon the ground, others ſticking into it a foot and two feet deep, in all directions. Part of one of the main poſts, about 10 feet long, ſuppoſed to be the N. W. corner poſt, with part of one of the plats of nearly the ſame length, and a brace which holds them together, were left ſticking in the ground, nearly perpendicular, to a great depth, in a field ſoutherly from the houſe about 8 rods diſtant. The boards and ſhingles of the houſe, with 3 or 4000 new boards, which lay by it, were ſo intirely ſhattered, that ſcarce a piece could be found above 4 or 5

5 inches wide, and vaſt numbers were not more than two fingers wide; ſome within the courſe of the wind, and ſome without, at great diſtances on both ſides of it (as were the timbers), ſticking in the ground, ſome nearly perpendicular, others inclining ſeverally towards almoſt all points of the compaſs.

What has been ſaid of the boards and ſhingles, is likewiſe true of the wooden furniture of the houſe: the tables, chairs, deſks, &c. ſhared the ſame fate; not a whole ſtick was to be found of any of them. Some of the beds, that were found, were hanging on high trees at a diſtance. Of the heavy utenſils, pewter, kettles, and iron pots, ſcarce any have been found. Some nails, that were in a caſk in the eaſt chamber, were driven, in great numbers, into the trees on the eaſtern ſide of the houſe. The ſhop and ſhed, before-mentioned, were torn in pieces, nothing of the ſhop remaining, but the ſills and floor; and a horſe ſtanding under the ſhed was killed. Another horſe, in a paſture at ſome diſtance from the houſe, on the eaſtern ſide, ran toward the houſe, as ſoon as the ſtorm was paſſed, trembling in an extraordinary manner, and preſently lay down and died. 'Tis ſuppoſed, he received ſome violent blow from ſome pieces of the houſe. The barn was thrown down, but its parts remained in a heap, without being diſperſed.

Such was the cataſtrophe of this houſe, which was effected in a very ſhort ſpace of time, as we learn from the teſtimony of one Warren, whoſe houſe ſtood about 50 rods eaſterly on the road. He ſays, that, upon hearing the wind, and ſeeing the rain beat into one of his doors, which looked toward

Lynde's houſe, he went and ſhut it; at which time, he ſaw the houſe and barn ſtanding; but going from it a little way, without bolting it, and reflecting, that it might blow open, he returned to bolt it; from whence, before he had proceeded croſs the room to bolt another door, that fronted the road, a large piece of timber from Lynde's houſe ſtruck the cap of the door laſt-mentioned, and burſt it open; ſo that from the time he ſaw the houſe ſtanding, to the time of his door's being burſt open, could hardly be more than a minute.

It is really extraordinary, that, in ſo ſudden and general a devaſtation, any perſons could eſcape with their lives. And yet the providence of God ſo ordered it, that but one life was loſt. There were, at that time, in the houſe fourteen perſons; Mr. Lynde, his negro man, nine women and children, and three travellers, it being a public-houſe; of all which, the negro only loſt his life. It is ſuppoſed, he was in the weſt chamber. He was found ſouth, a little eaſterly from the houſe, about 8 rods, lying acroſs a low wall, and a bed near him, which had been in the weſt chamber: his back, thighs, and arms, were broken, and he ſoon expired, in extreme miſery. His maſter, ſuppoſed to have been in the weſt lower room, was found nearly in the ſame direction, about 2 rods diſtant. He was winding his watch at that time; and the watch was found at one diſtance, and the caſe at another. The three travellers were found on the floor, near the ſouth door, which they had endeavoured to ſhut, much cut and bruiſed by the ſtones falling from the chimney, which lay round them. Three young women and a child were found unhurt on the kitchen floor, near to, and partly under, the eaſt door, which was blown down upon them, as they were endeavouring to ſhut it. The miſtreſs of the houſe, with a child in her arms, and two others, being in the kitchen, near a paſſage into the cellar, were forced down ſeveral ſtairs, where they were found; the woman being ſlightly hurt by ſome pieces of boards, which fell upon her. A child, ſtanding near the chimney, was buried in its ruins; but happily preſerved by a piece of board, which, falling obliquely againſt the jamb, ſecured it from the falling ſtones. Beſides the perſons in the houſe, there was a girl, about ſeven years old, before the ſouth door, the preſervation of whoſe life was not leſs remarkable. She was taken up from before the door by the wind, and carried above 30 rods. The people there are perſuaded, ſhe was carried over the tops of trees, being firſt ſeen running towards the houſe, in the edge of a thick wood, ſeveral rods from the courſe of the wind; having ſuffered no other injury, than breaking the collar-bone.

From the whole, it ſeems highly probable, that the houſe was ſuddenly plucked off from the ſills (to which the upright poſts are not faſtened), and taken up into the air, not only above the heads of the perſons, who were on the lower floor, but to the height of thoſe parts of the chimneys, which were left ſtanding, where, by the violent circular motion of the air, it was immediately hurled into ten thouſand pieces, and ſcattered to great diſtances, on all quarters, except that, from which the wind proceeded. And it farther appears, that the violence of the wind in that place was over, as ſoon as the houſe was taken up; otherwiſe, no body could have been left on the floor.

I have now given a very circumſtantial account of this furious blaſt; being perſuaded, that an attention to every particular in effects is generally neceſſary to a diſcovery of their cauſe. It appears to me ſo difficult to aſſign a cauſe adequate to theſe effects, to ſhew by what means a ſmall body of air could be put into a circular motion, ſo exceſſively rapid as this muſt have been, that I dare not venture any conjectures about it. It would be a great ſatisfaction to me, to know your ſentiments, or thoſe of any other learned gentlemen of the Royal Society, upon this article.

I beg leave to ſubſcribe myſelf,

With the greateſt reſpect,

Reverend Sir,

Your moſt obedient and

moſt humble ſervant,

John Winthrop.

Cambridge, New England,
30 July 1760.

IV. *A Letter from the Rev.* Nevil Maſkelyne, *A. M. Fellow of* Trinity *College, in the Univerſity of* Cambridge, *and F. R. S. to the Rev. Dr.* Birch, *Secretary to the Royal Society; containing a Theorem of the Aberration of the Rays of Light refracted through a Lens, on account of the Imperfection of the ſpherical Figure.*

Reverend Sir,

Read Jan. 22, 1761.

ABOUT two years ago, becoming acquainted with Mr. Dollond's curious diſcovery in optics, of correcting the aberration of the rays of light ariſing from the different refrangibility of the different ſorts of rays, by a combination of two different kinds of glaſs; and learning from him, in converſation, that he had invented a theorem, ſhewing the quantity of the aberration of the rays refracted through a lens, on account of the imperfection of the ſpherical figure; by the application of which, he was able to make the aberrations of the combined concave and convex object lenſes perfectly equal to, and conſequently to correct one another; I was deſirous of being more minutely acquainted with this farther great improvement in optics; and Mr. Dollond accordingly readily offered to gratify my curioſity. But in the mean while that he was looking over his papers, in order to lay them before me, having leiſure, I ſet about the inveſtigation of a ſimilar theorem myſelf; which having

completed,

completed, I interchanged with Mr. Dollond for his theorem, he taking mine, and I taking his. Our theorems, though ſimilar, were not exactly the ſame; but, by reduction to the ſame form, I inferred his theorem from mine; which gave me a farther confidence of the exactneſs of both.

I have here ſent you my theorem, and deſire, that you will lay it before the Royal Society.

I am, SIR,

Your moſt obedient

humble ſervant,

Nevil Maſkelyne.

Prince Henry, St. Helen's Road,
Jan. 16, 1761.

LET the form of the lens aſſumed, in the inveſtigation of the theorem, be a meniſcus, the radius of whoſe convex ſurface is greater than that of its concave ſurface; and the center of whoſe two ſurfaces lie on the ſame ſide of the lens, as the radiant point, from which the rays diverge, that fall thereon. The ray falling on the extreme part of the lens will, after refraction, diverge from a point before the lens, nearer thereto than the geometrical focus of rays diverging from the ſame radiant point, and paſſing indefinitely near the vertex.

Let Q expreſs the diſtance of the radiant point, before the lens, from its vertex; R, the radius of concavity of the ſurface, on which the rays firſt fall; and r, the radius of convexity of the ſecond ſurface; F, the principal focus, or the focus of parallel rays;

which

which will be on the ſame ſide of the lens, as the incident rays; becauſe R, the radius of the concave ſurface, is ſuppoſed leſs than r, the radius of the convex ſurface. Let the ratio of m to n be the ſame with that of the ſine of incidence to the ſine of refraction of rays paſſing out of air into glaſs, and let Y expreſs the ſemidiameter of the aperture of the lens; the angular aberration of the ray falling on the extremity of the lens, or the angle made between this ray, after being refracted through the extremity of the lens, and another ray or line, ſuppoſed to be drawn from the ſame extremity of the lens, to the geometrical focus of rays diverging from the ſame radiant point, and paſſing indefinitely near the vertex of the lens, expreſſed in meaſures of the arc of a circle to the radius unity, will be

$$\frac{\overline{m^3 - 2m^2n + 2n^3} \times Y^3}{\overline{m-n}^2 \times 2m \times F^3} + \frac{\overline{mn + 4n^2 - 2m^2} \times Y^3}{\overline{m-n} \times 2m \times F^2 r}$$
$$+ \frac{\overline{m+2n} \times Y^3}{2m \times F r^2} - \frac{\overline{4n^2 + 3mn - 3m^2} \times Y^3}{\overline{m-n} \times 2m \times QF^2}$$
$$- \frac{\overline{2m+2n} \times Y^3}{m \times QFr} + \frac{\overline{3m+2n} \times Y^3}{2m \times Q^2F}.$$

Where R, the radius of the firſt ſurface, is exterminated; and r, the radius of the ſecond ſurface, is retained:

Or, exterminating r, the radius of the ſecond ſurface, and retaining R, the radius of the firſt ſurface, the angular aberration is alſo expreſſed by

$$\frac{m^2 \times Y^3}{2 \times \overline{m-n}^2 \times F^3} - \frac{\overline{2m+n} \times Y^3}{2 \times \overline{m-n} \times F^2 R} + \frac{\overline{m+2n} \times Y^3}{2m \times F R^2}$$
$$+ \frac{\overline{3m+n} \times Y^3}{2 \times \overline{m-n} \times QF^2} - \frac{\overline{2m+2n} \times Y^3}{m \times QFR} + \frac{\overline{3m+2n} \times Y^3}{2m \times Q^2F}.$$

It may be proper to remark, that, as in theſe theorems, the principal focus is ſuppoſed to lie before the glaſs, as well as the radiant point, to adapt the theorem to other caſes, if the lens be of ſuch a form, as that its principal focus lies behind the glaſs, F muſt be taken negative: likewiſe, if the rays fall converging on the lens, or the point, to which they converge, lie behind the glaſs, Q muſt be taken negative: laſtly, if the firſt ſurface be convex, R muſt be taken negative; and if the ſecond ſurface be concave, *r* muſt be taken negative; and if, after all theſe circumſtances are allowed for, the value of the theorem comes out poſitive, the aberration is of ſuch a nature, as to make the focus of the extreme rays fall nearer the lens before it, than the geometrical focus, or farther from the lens behind it: but if the value of the theorem comes out negative, the aberration is of ſuch a kind, as to make the focus of the extreme rays fall farther from the lens before it, than the geometrical focus.

With reſpect to the application of this theorem to Mr. Dollond's combined object glaſſes, it is evident, that if the aberrations of the convex and concave lenſes added together (paying due regard to the ſigns of the theorem), are made equal to nothing, the two lenſes will perfectly correct one another: but as there are two unknown quantities unlimited in the equation, namely, the radius of one ſurface of each glaſs (for F and Q are given, as well as *m* and *n*), there is room for an arbitrary aſſumption of one of them, at the diſcretion of the theoriſt, or artiſt; which being done, there will remain a quadratic equation, whence there will reſult two values of the radius, which remains unknown, either of which will produce an aberration equal to that of the other lens.

V. *Extract of a Letter from the Abbé* De la Caille, *of the Royal Academy of Sciences at* Paris, *and F. R. S. to* William Watſon, *M. D. F. R. S. recommending to the Rev. Mr.* Nevil Maſkelyne, *F. R. S. to make at* St. Helena *a Series of Obſervations for diſcovering the Parallax of the Moon.*

Lincoln's-Inn-Fields, 8 Jan. 1761.

Read Jan. 8, 1761.

DR. Watſon lately received a letter from the Abbé De la Caille at Paris, in which he takes notice, "That although the parallax of the moon ſeems ſufficiently well determined, by the obſervations made in 1751, in "Europe and at the Cape of Good Hope; nevertheleſs, an element of this importance cannot be "too well aſcertained. He is of opinion, that Mr. "Maſkelyne's continuance in St. Helena may be "advantageouſly employed in making new obſervations; ſince the baſe, upon which theſe parallaxes "ſhould be calculated, ſhould exceed the earth's "radius.

"That if the Royal Society does approve of his "propoſition, and recommend to Mr. Maſkelyne "the execution of the ſcheme of correſpondence,

" which he has drawn up, he promiſes to comply " with it punctually on his part."

The Abbé has accordingly ſent Dr. Watſon a ſeries of obſervations, which he recommends to Mr. Maſkelyne to make, from the 13th of June 1761, a few days after the tranſit of Venus, till the 9th of May 1762. This paper Mr. Maſkelyne has tranſcribed, and propoſes to make theſe obſervations in concert with the Abbé De la Caille. And if a copy of this paper, which Dr. Watſon propoſes to lay before the Society, at their next meeting, was put into the hands of Dr. Bradley, that gentleman might likewiſe make correſpondent obſervations.

The Abbé likewiſe adds, " That he has ſuppoſed, " that the ſector, which Mr. Maſkelyne takes with " him to St. Helena, would take in five degrees and " a half on each ſide the zenith; and that his clock " would be regulated by ſydereal time." This ſector extends much beyond the Abbé's expectation, as it takes in eight degrees and a half on each ſide of the zenith.

The Obſervations recommended by the Abbé De la Caille to Mr. Maſkelyne.

1761.	Sydera Obſerv.	Culmin.			Decl. A.	
		H.	M.	S.	G.	M.
Jun. 13. Veſp.	λ ♍ - -	14	6	15	12	16
	α ♎ - -	14	37	42	15	2
	☾ - - -	14	39	0	13	54
	γ ♎ - -	15	22	13	13	59
Jun. 14. Veſp.	☾ - - -	15	30	0	18	27
	β ♏ - -	15	51	37	19	7
	ν ♏ - -	15	57	41	18	48

1761.	Sydera Obſerv.	Culmin.			Decl. A.	
		H.	M.	S.	G.	M.
Jun. 15. Veſp.	☾ - - -	16	19	0	22	3
	μ ♐ - -	17	59	31	21	6
Jun. 22. Mane.	γ ♑ - -	21	26	52	17	44
	δ ♑ - -	21	33	53	17	12
	☾ - - -	21	37	0	19	0
Jul. 10. Veſp.	λ ♍ - -	14	6	15	12	16
	☾ - - -	14	26	0	12	30
	α ♎ - -	14	37	42	15	2
Jul. 11. Veſp.	☾ - - -	15	12	0	17	12
	β ♏ - -	15	51	36	19	7
	ν ♏ - -	15	57	40	18	48
Jul. 12. Veſp.	β ♏ - -	15	51	36	19	7
	☾ - - -	16	2	0	21	0
	μ ♐ - -	17	59	30	21	6
Jul. 19. Mane.	☾ - - -	21	26	0	20	20
	γ ♑ - -	21	26	52	17	44
	δ ♑ - -	21	33	53	17	12
Jul. 20. Mane.	γ ♑ - -	21	26	52	17	44
	δ ♑ - -	21	33	53	17	12
	☾ - - -	22	16	0	16	24
	δ ♒ - -	22	42	0	17	7
Aug. 6. Veſp.	☾ - - -	14	7	0	10	40
	ζ Ophi -	16	24	4	10	4
Aug. 7. Veſp.	☾ - - -	14	57	0	15	50
	η Ophi -	16	56	45	15	24
Aug. 8. Veſp.	☾ - - -	15	48	0	20	5
	μ ♐ - -	17	59	30	21	6
Aug. 14. Veſp.	μ ♐ - -	17	59	30	21	6
	π ♐ - -	18	55	35	21	23
	☾ - - -	21	9	0	21	35

1761.

1761.	Sydera Obſerv.	Culmin.			Decl. A.	
		H.	M.	S.	G.	M.
Aug. 15.	γ ♑	21	26	52	17	44
Veſp.	δ ♑	21	33	53	17	12
16. Mane.	☾	21	59	0	17	45
Aug. 17.	δ ♒	22	42	0	17	7
Mane.	☾	22	49	0	13	0
Sept. 11.	γ ♑	21	26	52	17	44
Veſp.	δ ♑	21	33	53	17	12
	☾	21	40	0	19	20
Sept. 12.	☾	22	30	0	14	50
Veſp.	δ ♒	22	42	0	17	7
Oct. 8.	π ♐	18	55	35	21	23
Veſp.	☾	21	18	0	21	3
Oct. 9.	γ ♑	21	26	52	17	44
Veſp.	δ ♑	21	33	53	17	12
	☾	22	11	0	16	55
	δ ♒	22	42	0	17	7
Oct. 10.	α ♑	20	4	26	13	6
Veſp.	☾	22	58	0	11	55
Nov. 5.	γ ♑	21	26	52	17	44
Veſp.	δ ♑	21	33	53	17	12
	☾	21	51	0	18	50
Nov. 6.	δ ♑	21	33	53	17	12
Veſp.	☾	22	37	0	14	16
	δ ♒	22	42	0	17	7
Dec. 2.	☾	21	28	0	20	50
Veſp.	β Ceti	0	31	38	19	18
Dec. 3.	☾	22	17	0	16	12
Veſp.	β Ceti	0	31	38	19	18
Dec. 4.	☾	23	5	0	11	20
Veſp.	ε Ceti	2	28	5	12	54

1762.

1762.	Sydera Obſerv.	Culmin.			Decl. A.	
		H.	M.	S.	G.	M.
Feb. 14.	λ ♍ - -	14	6	17	12	16
Mane.	☾ - - -	14	17	0	13	0
Feb. 15.	α ♎ - -	14	37	44	15	2
Mane.	☾ - - -	15	11	0	18	5
Mart. 13.	☾ - - -	13	54	0	10	25
Mane.	λ ♍ - -	14	6	17	12	16
Mart. 14.	α ♎ - -	14	37	44	15	2
Mane.	☾ - - -	14	46	0	16	6
Mart. 15.	☾ - - -	15	40	0	20	0
Mane.	β ♏ - -	15	51	37	19	7
Apr. 10.	λ ♍ - -	14	6	17	12	16
Mane.	☾ - - -	14	18	0	13	25
	α ♎ - -	14	37	44	15	2
Apr. 11.	☾ - - -	15	16	0	18	48
Mane.	β ♏ - -	15	51	37	19	7
	ν ♏ - -	15	57	41	18	48
Maii 6.	☾ - - -	13	57	0	10	55
Veſp.	λ ♍ - -	14	6	17	12	16
Maii 7.	α ♎ - -	14	37	44	15	2
Veſp.	☾ - - -	14	50	0	16	35
Maii 9.	☾ - - -	15	44	0	21	20
Mane.	β ♏ - -	15	51	37	19	7

VI. *A Letter from the Rev.* Nevil Maſkelyne, *M. A. F. R. S. to* William Watſon, *M. D. F. R. S.*

Prince Henry, St. Helen's Road,
Jan. 17, 1761.

SIR,

Read Jan 22, 1761.

IN a letter which I wrote to you from this place, the beginning of this week, I deſired you would, in your anſwer to Abbé De la Caille, acquaint him, that I had propoſed to the Royal Society the obſervations of the moon's parallax, before his letter came; and that Dr. Bradley was to make obſervations at Greenwich, correſpondent to mine at St. Helena; and that I was drawing up a liſt of the proper obſervations to be made, and the proper ſtars with which the moon was to be compared, which I propoſed to tranſmit to the Abbé De la Caille, in order that he might attend to the ſame obſervations, if he thought proper. But as he has made out a liſt of proper opportunities of obſerving, I ſhall only ſet down five obſervations to be added thereto, which I beg you will tranſmit to the Abbé De la Caille; and likewiſe deliver a copy of the ſame to Dr. Bradley.

I alſo deſired in my letter, that you would deſire the Abbé De la Caille, and the other French aſtronomers by him, to attend to the obſervations of the eclipſes of Jupiter's ſatellites, eſpecially the firſt, from May 1761 to June 1762, incluſive, in order to ſettle the difference of longitude between Paris and St. Helena; which if it came in the name of the Society, it would be better; and that you would alſo deliver it as my requeſt to the Society, that they would recommend it to my Lord Macclesfield, Dr. Bradley, Mr. Raper, and Mr. Short, and any other gentlemen they know propoſe to attend carefully to the obſervation of the tranſit of Venus, to make as many obſervations of the eclipſes of the ſatellites as they conveniently can, in order to ſettle the difference of longitude between their place of obſervation and and St. Helena, in the moſt exact manner; which is of the utmoſt importance with reſpect to the uſe to be made of the obſervations of the tranſit of Venus.

We ſailed from hence laſt Tueſday; but the wind not holding, returned hither again the next day. We are now under way again; but doubt whether the wind will ſerve for us. I am ſorry to hear of the accident, which befel the Sea-horſe, though they came off victors. I hope it will not ſtop their voyage; for I learn, that if they ſail in a fortnight, they may ſtill be in time.

I am,

Your obedient

humble ſervant,

N. Maſkelyne.

1762.	Sydera Obſerv.	Culmin.	Decl. A.
		H. M.	° '
Jun. 2. Veſp.	☾ - - - γ ♍ - -	13 37 14 0	8 45 9 7
Jun. 3. Veſp.	☾ - - - α ♎ - -	14 30 14 37	14 36 15 1
Jun. 4. Veſp.	☾ - - - β ♏ - -	15 23 15 51	19 29 19 6
Jun. 30. Veſp.	☽ - - - α ♎ - -	14 16 14 37	12 59 15 11
Jul. 1. Veſp.	☽ - - - β ♏ - -	15 8 15 51	18 13 19 6

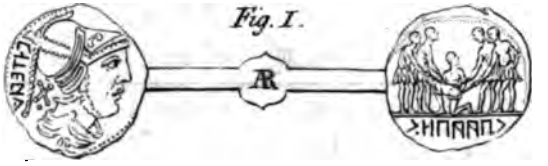
VII. *A Diſſertation upon a* Samnite *Denarius, never before publiſhed. In a Letter to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society, from the Rev.* John Swinton, *B. D. of* Chriſt-Church, Oxon. *F. R. S.*

Reverend Sir,

Read Jan. 15, and 29, 1761.

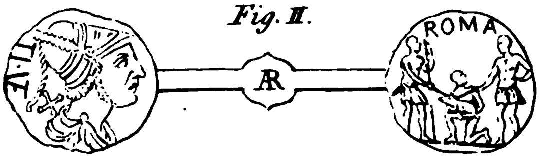
SINCE the communication of my laſt paper to the Royal Society, I have met with another Samnite denarius; which will, in a great meaſure, confirm what I endeavoured to evince in that paper. This inedited ſilver coin is adorned with two Etruſcan inſcriptions, that very well merit the attention of the learned. It is of the ſize of the larger conſular denarii, diſcovers much of the Roman taſte, and is in the fineſt conſervation. On one ſide it exhibits a galeated head, in all reſpects agreeing

Fig. I.



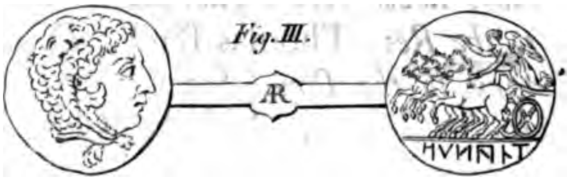
Penes Joannem Swinton, S.T.B. Oxoniens. R.S.S.

Fig. II.



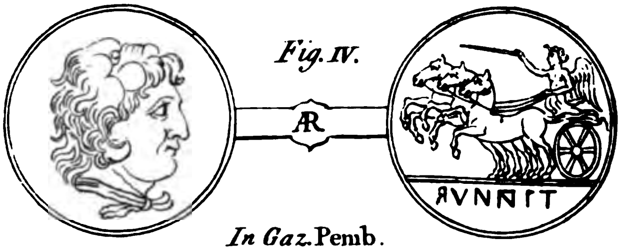
Penes Joannem Swinton, S.T.B. Oxoniens. R.S.S.

Fig. III.



Penes Joannem Swinton, S.T.B. Oxoniens. R.S.S.

Fig. IV.



In Gaz. Pemb.

J. Mynde sc.

agreeing with that preſerved by a (1) medal of the Veturian family, behind which ſtand the Etruſcan letters VNƎT⊢⊐, FITEEIV; none of which, except the laſt, and that but little, has ſuffered at all from the injuries of time. The type on the reverſe perfectly reſembles that preſented to our view by the poſterior part of the aforeſaid Roman coin, attributed to Tiberius Veturius; excepting that three human figures only occur on the latter of theſe pieces, and five on the former. The inſcription in the exergue is formed of the Etruſcan characters >.⊢IΠNNΠ.>, C. PAAPII. C. The workmanſhip of the Samnite denarius is ſo ſimilar to that of the Roman, that had not the legends, or inſcriptions, pointed out a different origin, theſe two medals might have been conſidered as ſtruck at the ſame place, on the ſame occaſion, and by the very ſame hand.

The Etruſcan elements VNƎT⊢⊐, FITEEIV, behind the galeated head, occupy the ſpace in which the Latin letters TI. VET. appear on the Roman denarius. This, in conjunction with what has been already advanced in favour of the ſame notion, amounts almoſt to a demonſtration, that the name of the pretor of the Marſi, as he is (2) called by Claudius

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(1) Vid. Vaill. Patin. & Sig. Haverc. in *Num. Fam. Vetur.*

(2) That the account of this general's death in the paſſage here referred to, as well as the ſtory of the two ſlaves immediately preceding it, was extracted out of Claudius Quadrigarius's *Annals*, there ſeems little reaſon to doubt. For that both theſe events happened in Italy about the ſame time, cannot well be denied. M. Lamponius having defeated a body of Roman troops, under the command of Licinius Craſſus, and put 800 of them to the ſword, ſhut the reſt up in Grumentum, a city of Lucania, either the firſt or ſecond campaign of the Social war, according

Quadrigarius, at the time of the Social war, was FITEEIVR, FITEEIVRI, FITEEIVRII, or VETVRIVS. Whence we may learn, that the VETTIVS of the antient authors, mentioned in my laſt paper, ought to be converted into VETVRIVS; and that the INSTEIVS (3) CATO of Velleius, as well as the T. VETTIVS (4) of Eutropius, was in reality the TI. VETVRIVS, whoſe name has been tranſmitted down to us by the conſular denarius. I ſay, " the " TI. VETVRIVS, whoſe name has been tranſmitted " down to us by the conſular denarius;" for that the Latin elements TI. VET. on the Roman coin ſtand for TIBERIVS VETVRIVS, and conſequently that the piece itſelf belongs to the Veturian family, my two Samnite medals render inconteſtably clear. A point therefore, by their aſſiſtance, is determined; which has been taken for granted only by Patin (5) and Vaillant, and not ſufficiently proved by Mr. Havercamp (6). The Etruſcan inſcription in the

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cording to Appian; and therefore then probably formed the ſiege of that place, mentioned by Seneca. And that the pretor of the Marſi was killed by his ſlave before the concluſion of this war, has been rightly obſerved by Lipſius. That learned man therefore ſhould not have conſidered the proximity of time of the two foregoing events only as a bare poſſibility, or rather a ſort of fiction, as he manifeſtly has done, in the following words: *Quid ergo? nonne bello Punico hoſtilia multa Italiæ loca, et in eo præſertim tractu?* POTUIT *et Sociali bello eveniſſe, ex Flori III. cap. xviii.*

Claud. Quadrigar. apud Senec. *De Benef.* Lib. iii. c. 23. Appian. Alexandrin. *De Bel. Civil.* p. 375. Juſt. Lipſ. *Comment. in Senec.* ubi ſup. p. 300. Antverpiæ, 1615. Vid. etiam Macrob. *Saturn.* Lib. I. p. 166. Londini, 1694.

(3) Vell. Paterc. Lib. ii.

(4) Eutrop. Lib. v. c. 3.

(5) Patin. & Vaill. ubi ſup.

(6) Sig. Haverc. *Comment. in Famil. Roman. Num. omn. &c.* p. 438—440. Amſtelædami, 1734.

exergue, >.ꟷIΠЯЯΠ.>, C. PAAPII. C, indicates the piece to have been ſtruck ſoon after C. Papius Mutilus had been conſtituted commander in chief of the Samnite forces, deſtined to act againſt the Romans, when the Italian ſtates took up arms againſt the republic, about the year of the city 663. From the preceding obſervations it appears, that our coin exhibits the names of two great generals, of different families, Tiberius Veturius and C. Papius Mutilus, in conformity to the cuſtom then (7) prevailing at Rome. For that ſuch a cuſtom prevailed there in this and the following age, ſeveral conſular medals of the Coponian, Conſidian, Curtian, Cocceian, Didian, Fonteian, Fufian, Mucian, &c. families leave us no manner of room to doubt.

With regard to the letters of which the firſt Etruſcan inſcription is compoſed, I have not at preſent much to ſay; having already given a particular deſcription of them, in a former paper. It may not be amiſs however to remark, that the ſecond of thoſe characters is apparently the Samnite-Etruſcan I, adorned with a ſort of accent; which has been taken notice of by the learned (8) Sig. Annibale degli Abati Olivieri, in his ſecond diſſertation. How that mark or accent varied the power of this element, I ſhall not preſume to decide; but that the ſound of it was thereby in ſome manner varied, I think cannot well be denied. The fourth letter is ſucceeded by a monogram, or complex character, formed of E and the aforeſaid accented I; as may be very fairly inferred from a ſimilar inſcription (9) on other denarii, that have preſerved the name of our Tiberius Veturius. The laſt element V is likewiſe an accented letter, having had originally a point or ſmall ſtroke, equidiſtant from each of its ſides, in the vacant ſpace between them; which has been flattened, and reduced to a kind of minute ſtrait line, almoſt contiguous to one of them, by the injuries of time. This accent undoubtedly pointed out the (10) diphthong OV, in like manner as did a ſmall curve line, joined to a ſide of V, though in another poſition, on ſome conſular (11) coins of the Furian and Pomponian families. That this mark or accent denoted the V to which it adhered to be equivalent to OV, on thoſe coins, is univerſally allowed; other conſular denarii exhibiting the word FOVRIVS for FVRIVS, and the cognomen MVSA being only the Greek term MOΥΣA or Mȣσα in Latin characters. That the laſt element of the Etruſcan inſcription now in view had originally a point or ſmall ſtroke between its ſides, is abundantly evident from the correſpondent letter (12) on other medals of Tiberius Veturius, and particularly one in the poſſeſſion of the Reverend and Learned Dr. Barton, Canon of Chriſt-Church, Oxon. and a worthy member of this Society. Hence it ſhould ſeem, that the cuſtom of accenting the V was derived from the Samnites by the Romans; and that the accent annexed to the V, on the conſular denarii, if not all the others that might have been in vogue amongſt the Romans, was of Samnite, or rather Etruſcan, extraction.

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(7) Patin. Vaill. & Sig. Haverc. in *Famil. Copon. Conſid. Curt. Cocc. Did. Font. Fuf. Muc. &c. Num.*

(8) *Saggi di Diſſertaz. Accademic. publ. let. nella Nobil. Accadem. Etruſc. dell' antichiſſ. Città di Cortona.* Tom. IV. p. 139, 146. In Roma, 1743.

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(9) Andr. Morel. *Theſaur. Numiſm. Fam. Incert.* Tab. I. num. 8, 9.

(10) Idem ibid. Had the name of the general of the Marſi been Vettius, the laſt letter here would have been equivalent to the Greek *Omicron*, or the ſimple Latin V. This is inconteſtably clear from the Greek word Βέττιος, or Οὐέττιος, anſwering to the Roman Vettius; which occurs both in Plutarch and Dio. But as the laſt element of the inſcription I am conſidering was indubitably pronounced OV, or OΥ; the name itſelf at length muſt have been Veturius, or Βετοῦριος, as we find it antiently written by ſome good authors. This ſingle obſervation, excluſive of others, that might be offered, to demonſtration evinces the point formerly deduced from the appearance of the Etruſcan ɑ on another Samnite-Etruſcan coin. The inſcription therefore exhibited by that medal and the legend before me mutually ſtrengthen and ſupport each other.

Plut. in *Grach.* Dio, Lib. xxxvii. p. 48. B. & Lib. xxxviii. p. 63. E. Edit. Wechel. Hanoviæ, 1606. Plut. in *Num.* Dionyſ. Halicarnaſ. *Antiquit. Roman.* Lib. ix.

(11) Vaill. Patin. & Andr. Morel. in *Famil. Roman. Fur. & Pompon. Num.*

The Etruſcan legend in the exergue has one letter more than the ſimilar inſcription on the coin of C. Papius Mutilus, explained by Sig. (13) Annibale degli Abati Olivieri, in his ſecond diſſertation. That letter is the Samnite-Etruſcan accented I, being the laſt element of the name .𐌝I𐌐𐌀𐌀𐌐, PAAPII. After what has been advanced on this head by the learned gentleman juſt mentioned, ſcarce any thing is left for me to ſay upon the ſame ſubject. It may not be improper however to obſerve, that this character is exhibited as repreſenting I by other (14) Samnite-Etruſcan remains of antiquity. Amongſt the Dorians it ſeems to have anſwered to the (15) Æolic *Digamma*, and the Greeks in general are ſaid to have (16) uſed it ſometimes as a note of aſpiration. On the *Marmor Sanduicenſe* (17), according to Dr. Taylor, it ſerved to expreſs the value of the obolus. That the Samnite-Etruſcan I, in the word MVTIL, 𐌋𐌉𐌕𐌖𐌌, on one of C. Papius Mutilus's coins, is accented, appears likewiſe from the correſpondent word Μετίλος in Appian (18), where the I has apparently an acute accent over it. The ſame mark of the denarius here preſents itſelf to our view that occurs on the ſimilar Roman coin. This ſeems plainly to imply, that the notation of the Samnites agreed with that of the Romans, at the time of the Social war; which, indeed, ſufficiently appears from other inſtances that might be produced. The name .𐌝𐌉𐌐𐌀𐌀𐌐, PAAPII, is undoubtedly expreſſed in the nominative caſe, as the Roman names on the conſular denarii are; at leaſt, every thing conſidered, I cannot help thinking this extremely probable. Nor will the point, at the end of the word, as I apprehend, diſcountenance ſuch a notion; ſince a point is ſometimes viſible after a complete name, or ſurname, as well as after a part of ſuch name, or ſurname, on ſeveral pieces of the Poſtumian, Pomponian, &c. families. Of this the inſcriptions A. POSTVMIVS. COS, ALBINVS. BRVTI. F, SVLLA. COS, on coins now in my poſſeſſion, are clear and inconteſtable proofs. I therefore hope the learned will indulge me the liberty of ſuppoſing, that the name .𐌝𐌉𐌐𐌀𐌀𐌐, PAAPII, is here complete; eſpecially, as the Etruſcan termination II ſometimes anſwered to the Roman or Latin termination IVS, as we learn from (19) Sig. Paſſeri. The concluſions to be drawn from hence are too obvious to be, at this time, either mentioned or inſiſted upon.

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(12) Andr. Morel. ubi ſup.

(13) *Saggi di Diſſertaz. Accadem. di Corton.* Tom. II. p. 49. in Roma, 1738. & Tom. IV. p. 132. In Roma, 1743.

(14) Jo. Bapt. Paſſer. Piſaurenſ. *Junonal. Sacr. Menſ. Illuſtrat.* Vid. etiam *Numiſm. Antiqu. &c.* Thom. Pembroch. et Mont. Gomeric. Com. P. 2. T. 88. num. 3.

(15) Tayl. *Comment. ad Marm. Sanduicenſ.* p. 43, 44, 45. Cantabrigiæ, 1743.

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(16) Serg. Max. Victorin. Quintilian. Lib. i. c. 4. Donat. Priſcian. Cledon. &c. Vid. Grammaticos Putſchii, Col. 1829, 1943, 1742, 1287, 1889, &c.

(17) Tayl. *Comment. ad Marm. Sanduicenſ.* p. 48, 49, 50. Cantabrigiæ, 1743.

(18) Appian. Alexandrin. *De Bel. Civil.* Lib. i. p. 381.

Before I diſmiſs the preſent ſubject, I ſhould beg leave farther to remark, that the ſpace behind (according to the Etruſcan manner of writing) the laſt prenomen in the exergue, >, C, on a (20) coin publiſhed by Sig. Annibale degli Abati Olivieri, on one in the Rev. Dr. Barton's cabinet, on another in my ſmall collection, and on that I am conſidering, is capacious enough to contain an element, or at leaſt a part of one, of the Samnite-Etruſcan alphabet; and yet that not the fainteſt traces of any part of one are viſible in this ſpace. Hence I am inclined to believe, that there never was a letter there. Which if we admit, it will ſeem to follow, that the Samnites and Etruſcans, at leaſt in the ſeventh century of Rome, wrote only >.⊦IΠЯЯΠ .>, C. PAAPII. C, equivalent to C. PAPIVS. C, or CAIVS PAPIVS CAII, imitating the Greeks in this particular; who uſed the father's name in the genitive caſe only, ſuppreſſing by a moſt common (21) ellipſis the word ΥΙΟΣ, on ſuch occaſions, as by an infinity of inſtances might be evinced, were it in any manner neceſſary.

Some authors (22) take the galeated head, exhibited by the medal I am conſidering, or at leaſt by that of Tiberius Veturius above-mentioned, to have repreſented *Fortitude* or *Valour*; which was eſteemed as a deity, by the Romans. But Sig. Annibale degli Abati Olivieri believes a ſimilar galeated head (23), on one of the medals of C. Papius Mutilus, to have pointed out to us Mars, the god of war. Which if we admit, that preſerved by our coin will probably be allowed to have repreſented the ſame deity. Nor can I think this at all remote from truth, as Mars was held in the higheſt veneration amongſt the antient inhabitants of Italy, and particularly the (24) Samnites, to whom the piece before me ought indubitably to be referred. Be this as it will, the galeated heads ſtill viſible on my Samnite-Etruſcan denarius, in the fineſt conſervation, and the Roman one of the Veturian family, here deſcribed, ſeem in all points to have agreed, notwithſtanding the diverſity of characters on thoſe coins, as has been already obſerved.

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(19) Jo. Bapt. Paſſer. Piſaur. *Junonal. Sacr. Menſ. Herculanenſ. Illuſtrat.* Vid. *Symbol. Litterar.* Vol. I. p. 207. Florentiæ, 1748.

(20) Annib. degli Abati Olivieri, in *Saggi di Diſſertaz. Accademic. &c.* Tom. II. p. 49. & Tom. IV. p. 132.

(21) Such ellipſes as this were antiently not uncommon amongſt the Etruſcans, as we learn from ſeveral inſcriptions in the language and character of that nation; for a farther account of which, recourſe may be had to the author here referred to. Jo. Bapt. Paſſer. *De Architect. Etruſc.* apud Ant. Franc. Gor. in *Muſ. Etruſc.* Vol. III. p. 121, 122. & alib. Florentiæ, 1743.

(22) Fulv. Urſin. Patin. Vaill. Sig. Haverc. &c.

The four armed ſoldiers touching with their ſwords, or ſticks, a ſow-pig, held by an herald, with his left knee upon the ground, on the reverſe, undoubtedly point out to us an alliance, or confederacy, formed between four powerful Italian ſtates. The ceremony repreſented by this type was more antient, (25) according to Livy, than the reign of Hoſtilius Tullus, the third king of Rome. We find it deſcribed by (26) Virgil, who agrees with Livy in this particular, in the following lines:

Armati, Jovis ante aras, paterasque tenentes,
Stabant; et cæsâ feriebant fœdera porcâ.

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(23) Annib. degli Abat. Olivier. ubi ſup. Tom. II. p. 64, 65.

(24) Annib. degli Abat. Olivier. ubi ſup. Tom. II. p. 64, 65.

(25) T. Liv. Lib. i.

(26) Virg. *Æn.* Lib. viii. v. 640, 641.

The Samnites, the Marſi, the Picentes, and the Lucanians (27) ſeem to have been the four principal nations that firſt entered into an alliance againſt Rome, the memory of which has perhaps been perpetuated by this coin, a little before the commencement of the Social war.

That the firſt letter of the monogram, in the inſcription behind the galeated head on my Samnite denarius, is E, to demonſtration appears, from the laſt Etruſcan element of the word]ᗡƎ†V†, TVTERE, as it occurs on a valuable medal publiſhed by (28) Mr. Havercamp; eſpecially, as this is ſupported by a ſimilar inſcription, handed down to us by one of Lord (29) Pembroke's Etruſcan coins. The laſt letter of the aforeſaid monogram is evidently the Samnite-Etruſcan accented I, as has been already remarked. This character and the other connected with it formed the diphthong IƎ, or EI, which the moſt antient (30) nations of Italy had in common with the Greeks. A coalition of the two elements here is moſt evident and conſpicuous. The accented I in power ſeems to have approached pretty near the Latin E, as we learn from the name VETVRIVS; the ſecond letter of which apparently anſwers to the firſt Samnite-Etruſcan accented I of the word VИƎT⊦⅃, FITEEIV, on my coin. It may likewiſe be clearly evinced, from the local proper name ᑋVИИ⊦T, TEANVR, or TEANOR, exhibited by a ſilver medal of Teanum, in my poſſeſſion, extremely well preſerved:

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(27) Aut. *Liv. Epit.* Lib. lxxii, lxxiii. Appian makes the Marſi, the Peligni, the Veſtini, and the Marrucini to have firſt taken up arms againſt the Romans, in the Italian war. Appian. Alexandrin. *De Bell. Civil.* Lib. i. p. 634. (374) Amſt. 1670.

(28) Andr. Morel. ubi ſup. Tab. Num. Hiſpan. n. 18.

(29) *Numiſm. Antiq.* &c. à Thom. Pembr. & Mont. Gomer. Com. *Collect.* P. 3. T. 116. num. 1.

(30) Jo. Bapt. Paſſer. Piſaurenſ. ubi ſup. Vid. *Symbol. Litterar.* ubi ſup. p. 207.

preſerved. This piece, about the ſize of a double denarius, preſents to our view a type on the reverſe ſomewhat different from that of Lord Pembroke's (31) ſimilar medal. The Samnite-Etruſcan N and R, on my coin, are of a pretty unuſual form. With regard to the word .𐌝𐌉𐌐𐌀𐌀𐌐, PAAPII, or PAPIVS, we find a Samnite proper name, with the ſame termination in the nominative caſe, on one of the moſt valuable Samnite-Etruſcan remains of antiquity, dug out of the ruins of Herculaneum, and moſt learnedly explained by Sig. (32) Paſſeri. That word and the prenomen prefixed to it occur in the following Samnite-Etruſcan characters, .𐌉𐌉𐌁𐌀𐌋𐌔.𐌋, L. SLABIVS, not L. SLABIIS (33), as that acute and ingenious author ſeems once to have imagined. That ſome Etruſcan proper names, of cities at leaſt, in nature ſingulars, and expreſſed in the nominative caſe, always retained the termination II, from the words VEII, TARQVINII, VOLSINII, &c. muſt be allowed abundantly clear.

Many more curious particulars, relative to antient Etruſcan literature, are naturally deducible from the Samnite-Etruſcan inſcriptions I have undertaken to conſider, in this and a former paper; all of which I muſt at preſent ſuperſede, as having time only to aſſure you that I am,

Good Sir,
Your moſt obliged,
and moſt obedient ſervant,
J. Swinton.

Chriſt-Church, Oxon.
July 21, 1760.

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(31) *Numiſm. Antiq. &c.* à Tho. Pembr. & Mont. Gomer. Com. *Collect.* P. 2. T. 88. num. 3.

(32) *Symbol. Litterar.* ubi ſup. p. 207—216.

(33) Jo. Bapt. Paſſer. Piſaurenſ. *Junonal. Sacr. Menſ. Herculanenſ. Illuſtrat.* Vid. *Symbol. Litterar.* ubi ſup. p. 209.

VIII. *An Account of an Eruption of Mount* Veſuvius: *In a Letter to* Philip Carteret Webb, *Eſq*; *F. R. S. from Sir* Francis Haſkins Eyles Stiles, *Bart. F. R. S.*

Dear Sir, Naples, 23d Dec. 1760.

Read Jan. 29, 1761.

I Did not intend to have paid my reſpects to you, till I had better leiſure; but a ſudden eruption of Veſuvius this day prompts me to give you a few lines by this poſt. The mountain, which was quiet in the morning, with ſcarce any viſible ſmoke, threw up on a ſudden, about noon, a vaſt column of black ſmoke, which roſe to a very conſiderable height; and, before it had diffuſed itſelf, made a ſplendid and glorious appearance, as the ſun, which was then ſhining, gilded the ſuperior part of it; but ſoon after, it diſperſed, and covered all the mountain, and a great portion of the ſky in that quarter. The aſhes, that fell from it, reſembled the falling of a heavy ſhower, ſeen at a diſtance, and muſt have done great miſchief, if any living thing was under them, as is but too probable. The drift of the ſtorm, if I may call it ſo, was towards the ſouth-eaſt, the wind being, I believe, nearly north-weſt. Portici might be within its influence; but the body of the ſmoke ſeemed to go beyond it; I mean, that it paſſed on the ſouth-eaſt ſide of it, which is beyond it, reckoning from Naples. At the ſame time that this ſmoke broke out, we obſerved two large columns of ſmoke ariſing at the foot of the mountain, on the ſouth-eaſt ſide of it which beſpoke eruptions in that part: and this has proved true; for the firſt ſmoke from the top ſoon after decreaſed, probably from the vent obtained at the foot; and ever ſince ſunſet, we have ſeen the foot all on fire. It is now burning with great violence in that part, it being about eleven o'clock at night. The direction of the line of fire, as we ſee it, is from the mountain towards the ſea, and runs, as we judge here, in that part, where Pliny's Lava, as it is called, came down to the ſea. The preſent lava cannot, we think, be far from the ſea. We ſuppoſe, that the mountain has burſt in its ſide, ſomewhere much nearer the ſummit; but that the lava has run down under the old lava, till it broke out where the fire now is. The line of fire, we think, muſt be two, if not three, miles in length.

Mr. Lowther, and his companion Mr. Watſon, were at the mountain, when the ſmoke broke out at the ſummit, and had almoſt climbed its height; but were fortunately to the windward of it, or they muſt have been deſtroyed. The noiſe, they ſay, was ſhocking to them, and the ſtones thrown up very alarming. Their guides fled firſt, and they after them; and they have eſcaped all harm, but the fatigue. As the poſt ſets out in a quarter of an hour, I can only haſtily aſſure you of my being truly

Yours, &c.

F. H. Eyles Stiles.

IX. *An-*

IX. *Another Account of the ſame Eruption of Mount* Veſuvius: *In a Letter to* Daniel Wray, *Eſq; F. R. S. from Sir* Francis Haſkins Eyles Stiles, *Bart. F. R. S.*

Dear Sir, Naples, 29th Dec. 1760.

Read Feb. 5, 1761.

ALL public exhibitions are prohibited for a few days, on account of the eruption of Veſuvius; and intereſt is making with the ſaint of the place, to protect the city from the miſchiefs, which the mountain is ſuppoſed to be threatening us with. There is, indeed, a very extraordinary eruption at or near the foot of the mountain; but it bodes no evil to Naples in the opinion of any, but the very timorous, who take in all poſſibilities, and who are led to imagine, from this eruption at ſuch a diſtance from the ſummit, that the ſoil, on which we ſtand, is not to be truſted. This new eruption began on the 23d inſtant: it was accompanied by a very extraordinary one at the ſummit, which I was an eye-witneſs of, from our own windows, about noon; and, I believe, this was a very few minutes after it happened. Mr. Lowther, and his companion Mr. Watſon, were, at that time, climbing the mountain, and, with the Abbate Clemente their antiquary, and ſome ruſtic guides, were arrived within fifty yards of the ſummit, when it burſt out. The flames, and the accenſed ſtones thrown up, were very terrible, by their account; ſome of the latter, as large as foot-balls, fell on their ſide; but the greater part fell on the other ſide the mountain.

The ſmoke only was viſible from our windows, the flames being concealed within the ſmoke, and alſo overpowered by the brightneſs of the ſunſhine. But this ſmoke was a moſt glorious object: for it formed an upright column, of a very great thickneſs, at firſt; but ſenſibly increaſing every moment, by freſh ſmoke, that we ſaw climbing the ſides of the column, as if the interior part was too ſolid to admit it. The height of this column anſwered in proportion to the diameter, like that of a pillar in architecture. From this you may judge of the bulk of the appearance: the column ſupported its perpendicularity near a quarter of an hour, whether from the ſtrength of the blaſt that threw it up, or from the reſiſtance made by ſo great a body to the force of the wind; perhaps from both theſe cauſes; for the latter muſt be admitted as one, if we conſider, that the power of the wind will only increaſe with the ſurface of the body to be moved, whereas the reſiſtance will be as the maſs. The upper part of this ſmoke was finely illumined, and variegated, by the ſun; and when it began to unfold itſelf, it appeared juſt as Pliny has deſcribed the eruption, that deſtroyed the naturaliſt; that is, like a branching tree; to which compariſon of his I may add this circumſtance, that the creeping of the freſh ſmoke up the ſides perfectly reſembled the undulating motion of a neſt of caterpillars, when climbing the trunk of ſome vegetable. This glorious ſight, which is itſelf almoſt worth a journey from England, did not laſt long; for, in leſs than an hour, it diffuſed itſelf, blackening all the mountain, and a large portion of the ſky; and when the wind had cleared the top of the mountain, which it did ſoon after, we obſerved the ſmoke aſcending from it to be very moderate, though, if compared with that, which iſſued before the eruption, it might be ſaid to be very conſiderable.

Thus far the ſummit: now for the foot, where we obſerved, at the ſame time, a double column of ſmoke, that we judged to be an eruption, and it proved to be a very great one. The flames, and the light of the ſtream of lava that iſſued from it, became viſible after ſunſet. We went all of us the next morning (the 24th), to take a nearer view of the eruption; we took the great road to Salerno, and about ten miles from Naples, about mid-way between Torre del Greco and Torre del Annuntiata, we were ſtopped by the ſtream of lava, which had croſſed the road, and was making for the ſea. The mouths of the eruption were about a mile and half, or better, to our left, and were raging in a very frightful manner, as the noiſe of the exploſions, which ſucceeded one another, at the interval of only a ſecond or two, was equal to a ſtorm of thunder. The flames were very bright, after it was dark; and the accenſed ſtones, which were thrown up in vaſt quantities at every exploſion, reſembled the ſpringing of a mine, as they call it, in a fire-work. We ſtaid an hour or two, in the night, on the ſpot, to behold this ſight. Theſe mouths of fire ſtill continue to play; but the lava has not yet reached the ſea, though it was ſaid to be within half a mile of it, when we were there. A ſmall riſing of the ground before it has obliged it to ſpread in breadth, and its progreſs for the ſhore is very ſlow: perhaps it may not reach it, if the eruption continues, but may, by the level of the ground, be determined to ſome other direction. The mouths are ſaid to have been fourteen in all at firſt, afterwards reduced to eight, and now, I believe, much fewer. There are three hillocks, large enough to be diſtinguiſhed at Naples, that are formed by the ſtones and matter thrown up at theſe mouths, and one of them is already a young mountain. Some imagine the eruption will laſt many months, as the lower eruptions have generally laſted longeſt; and this, I think, is a great deal lower than any that ever happened.

F. H. Eyles Stiles.

X. *Extract of a Letter from Mr.* Robert Mackinlay, *to the Right Hon. the Earl of* Morton, *F. R. S. dated at* Rome, *the 9th* January 1761. *concerning the late Eruption of Mount* Veſuvius, *and the Diſcovery of an antient Statue of* Venus *at* Rome.

Read Feb. 19, 1761.

THERE has been a moſt terrible eruption lately of mount Veſuvius, about the latter end of laſt month; but the accounts hitherto arrived are not very diſtinct: however, they all agree, that there were nine new mouths, or openings, towards the Torre del Greco and Annonciada: that very conſiderable ſhocks of an earthquake were felt all over Naples: that neither fire nor ſmoke came out of the old crater: that the lava had run into the ſea.

ſea: and that beyond Portici, upon the high-road, the lava was in height ſeventeen palms, and ſome of the ſtreams four hundred yards broad. Much damage has been done to houſes and vineyards; and 'tis ſaid the palace of Portici has ſuffered ſomewhat.

In the month of September laſt, a Venus, of moſt exquiſite workmanſhip, was dug up here in the Mons Cœlius, near the place called Clivo Scauri. It is in the poſſeſſion of the Marquis Carnavallia, who gave fifty ſcudi to the workmen, their full demand, as the half of the value, according to agreement, though 'tis worth ſome thouſands. It is full ſix feet high, in the ſame attitude with the Venus of Medicis, with this difference, her right-hand before her breaſt, and her left ſupporting a light drapery before the pudenda. On the baſe, which is of one piece with the ſtatue, and quite intire, is the following inſcription:

ΑΠΟΤΗϹ
ΕΝ. ΤΡΟΙΑΔΙ
ΑΦΡΟΔΙΤΗϹ
ΜΗΝΟΦΑΝΤΟϹ
ΕΠΟΙΕΙ.

XI. A

XI. *A Letter to the Rev. Dr.* William Brakenridge, *Rector of* St. Michael Baſſiſhaw, London, *and F. R. S. concerning the Term and Period of Human Life: In which the Inequalities in conſtructing, and the falſe Concluſions drawn from Dr.* Halley'*s* Breſlau *Table are fully proved; the ſuppoſed extraordinary Healthfulneſs of that Place is particularly examined, and confuted; and its real State equalled by divers Places in* England; *the Imperfection of all the Tables formed upon* 1000 *Lives is ſhewn; and a Method propopoſed to obtain one much better: By* T. W. *A. M.*

" It were to be wiſhed, that ſome inland town could be found in
" England, where there was kept an annual regiſter of births
" and burials, with the ages of the deceaſed, and where there
" is no confluence of ſtrangers."
Dr. Brakenridge, in Phil. Tranſ. Vol. XLIX. p. 172.

" An author ſhould be fond of reading his works to thoſe, who
" know how to correct, and eſteem them.—He that will not
" be corrected, or adviſed, in his writings, is a-kin to a pe-
" dant." Monſ. De la Bruyere, tranſl. by N. Rowe, Eſq;

Quid dignum tanto feret hic promiſſor hiatu? Hor. de Art. Poet.

Reverend Sir, 1760.

Read Jan. 29, & Feb. 5, 1761.

AS I highly honour your ingenuity, and applaud your candid treatment of thoſe, who entertain ſentiments different from your own, while they attempt to diſcover uſeful truth, I preſume to lay before you, a great maſter of the ſubject on which I write, ſome hints relating to what has been publiſhed, and hope to give, as has been deſired (1), a little further light, by what I have obſerved from my own pariſh for 24 years paſt, having noted the particular ages of 1700 perſons buried in that time (2), from the London bills, more eſpecially on the age of 100 years, and upwards, and from the accounts of every other place I could procure. I ſhall mention at preſent a few leading points only, and thoſe as briefly as I can, ſenſible of your ability to trace their conſequences, and preſent them as they occur to mind, on view of your table and diſcourſe in the Philoſophical Tranſactions, Vol. XLIX. p. 167.

Comparing the burials of London and Breſlau, you ſay 8110 die at London, and 202 at Breſlau, under 2 years of age. I acknowlege it not only yours, but a current opinion, taken as a firſt principle, that at Breſlau about $\frac{1}{5}$ of thoſe that are born die under that age; and the place has been celebrated for its healthfulneſs, for the ſucceſsful care of infants in particular, and for the good conſtitution and longevity of its inhabitants in general, a place much envied, and much contended for. Now I grant, that the numbers 145 and 57 make 202; and that 1000 — 202 = 798, which ſtand in the third year in Dr. Halley's table; but ſtill can by no means admit the inference (and am ſurprized it ſhould ever be imagined, that of 1000 children born at Breſlau, 202 only die under 2 years of age), for very good reaſons, to be found in the book of nature, and in Dr. Halley's diſſertation too, as I ſhall make appear by and by.

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(1) Richards' Annuities on Lives, 1739, pref. iii. Dr. Brakenridge, Phil. Tranſ. Vol. XLIX. p. 172,

(2) Intending, if I ſhould live, to publiſh them, with extracts from the regiſters for 200 years, and the reſult of the number of inhabitants twice taken from houſe to houſe.

In the mean time, let me obſerve how much it were to be wiſhed, that all, who write upon this ſubject, would begin from the birth, or 0 year, and give a true annual regiſter of the growing, the moſt confirmed, and the declining, ſtate of life, by ſome method deviſed to make it viſible at once, as I ſhall ſhew hereafter, without leaving the reader to try the numbers ſingly upon every occaſion. For while ſome account from the quick conceptions (3), as in the London bills; others from the living births, as is your way, and I think the beſt; others from 6 or 9 months, or a full year after the birth, as Dr. Halley and Mr. Kerſeboom have done, (and great is the tranſition from 0 year, or the birth, to thoſe of 1 year old), there muſt ariſe confuſion at firſt ſetting out, and apparent, if not real contradictions, in comparing one account with another. And it ſhould likewiſe be well remembered, that if a leſs number are taken for the deaths in the firſt ſtage of life than there ought to be, the more in courſe are thrown (4) back upon the enſuing decads of years; and thus a whole table may be effected by the firſt year only unfairly repreſented, as Dr. Halley's certainly is, if he ever meant, as he is generally, but I think not rightly, underſtood.

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(3) Graunt's Obſervations on the Bills of Mortality, 5th edit. 1676, p. 22. 84, 85. and from hence abortives and ſtilborn are included in the burials.

(4) See at the end of this Letter, p. 69. (*c*), (*d*), (*e*), and (*f*).

I ſaid I could not admit, that of 1000 children born at Breſlau 202 only died under 2 years of age; and, having prepared you for it, ſhall now give you the reaſon. Dr. Halley ſtates the births (5) certain at 1238, and adds farther, that 348 of them do die in the firſt year; that but 890 arrive at a full year's age; and that 198 more do die in the 5 years next following. Upon which data, I ſtate thoſe 6 years of the local lives and deaths, and their proportionals for 1000 births, as follows.

Year begin.	Born.	Die.		Born.	Die.	
1	1238	348		of 1000	281	
2	890	76	198	719	61	160
3	814	49		658	40+	
4	735	35		618	28	
5	730	23		590	19+	
6	707	15		571	12	
7	692, as in the table; but 559 only, if begin with 1000 births.					

+ *ſignifies an unit taken in to the integers from a decimal of .5 or more.*

And from hence I think it very plain, that out of 1238 births at Breſlau, 424 die under 2 years old; and therefore, out of 1000, 342 die under that age: which is ſomewhat more than I obſerve in my pariſh, though mine exceeds Mr. Kerſeboom's (6) account; and I am better pleaſed to find, that what I take for the truth lies between them. And here I ſhall venture to aſſure you, upon the whole, that when brought to the due proportionals for 1000 births only, the account of Breſlau falls in between (7) thoſe of my pariſh and of All-Saints in Northampton; ſo that in reality, there is nothing either remarkably (8) healthful, or long-lived, in the inhabitants of Breſlau, as has been imagined, by miſtaking our author's meaning, who perhaps might intend his ages ſhould imply the middle of every year, his title being age current, and from the 1238 threw off 238 only for 6 months, or more, at firſt ſetting out, and not the whole 348, as he would have done, if intended for the whole year. However, let his deſign be what it will, the number 692 (being the remainder of 1238 after 6 years deaths) is placed in his table; and if we take the 692, and what follows thenceforth, we muſt not forget with what number the author began, nor confound his with other tables, that really begin with 1000 births, becauſe this has 1000 perſons in the firſt year.

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(5) Lowthorp's Abridgment, 5th edit. 1749, Vol. III. p. 669.

It is with great regret, that I mention any blemiſhes in this table, ſo much and deſervedly eſteemed, which has given the lead to many others of the like

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Year. Lives. Die.
(6) Mr. Kerſeboom's table begins 1 — 1125 — 50; but the firſt year from the birth is ſuppreſſed, and 0 year ſhould be 1400 — 275. He might have ſaid this plainly, to prevent any ſtumbling at the threſhold.

(7) See p. 69. (*d*), (*b*), (*k*), and (*l*).

(8) Ward's Clavis Aſuræ, p. 111. Hodgſon, pref. Stonehouſe, pref. p. 7.

kind, but I cannot ſay it is exact. For by departing from his preliminary (9) diſcourſe in the firſt 6 years, and varying his table from the ſtated proportions, at their reſpective ages therein ſet forth, it is certain the one does not agree with the other, till the years 57 to 62, and in the ſingle years 72 and 80. And it were much to be wiſhed, that Dr. Newman's papers were made public, to diſcover the origin and juſtneſs of thoſe proportional numbers, how they prove from year to year, and from what leading proportions they were deduced.

However, if the conſideration of the firſt year was the only reaſon for deſiring them, it might be needleſs, there being another paſſage in the ſame diſſertation, which further confirms what I aſſert; where, ſpeaking of theſe tables of Dr. Newman, about to be ranged in particular form by himſelf, he makes this obſervation (10). It appears hereby, that the one half of thoſe that are born, die in 17 years time, 1238 [births] being, in that time, reduced to 616. For we cannot, by any means, apply theſe words to the number 1000 in his own table, which are not reduced to half, till near the 34th year; an aſſurance to all, that know any thing of theſe matters, that 1000 births, and 1000 perſons, the one in the beginning of life, the other in the beginning of his table, mean very different things, and that any concluſions, truly drawn from the one, may be very falſe when transferred to the other.

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(9) A general ſtate of theſe may be ſeen, p. 69. a yearly one would take up too much room here, though I have it before me.

(10) Lowthorp's Abridgment, Vol. III. p. 677.

If

If ſome miſtakes, and unfair compariſons, ariſe from divers tables not beginning all from the ſame point; if in the London accounts the abortive and ſtilborn muſt be thrown out, and alſo an allowance made for as many as die in the greater part of the firſt year (while the 1238 Breſlau births were reducing to 1000 perſons only), before a juſt compariſon can be made with Dr. Halley's table, as it now ſtands; then the 8110 at London (11) muſt be reduced, or elſe the 202, or rather the 342 abovementioned, be further increaſed by an allowance for ſuch abortive and ſtilborn. And when theſe alterations are made, the accounts of the two places will not appear ſo amazingly different, in proportion to their reſpective numbers, upon the whole.

Beſide the inconvenience of the various accounts not beginning together, I ſhall add another objection no leſs material; that the tables are formed in too ſmall numbers, and, by that means, cut off 20 or 30 years of the term of life, and undervalue it in annuities, as nothing worth. He that begins with 1000 only, either ſtops ſhort of 90, or runs quite out between the 95th and 100th year, and can go no further; becauſe out of 1000 births, it is not expected, ſo much as one ſhould arrive at the age of 100 years. But what muſt become of thoſe many in the London (12) accounts (for inſtance, in the 30 years, 1728 to 1757 incluſive) 2979 living at 90, 242 living at 100, 10 living at 110, and one living to 138? Are theſe to be wholly omitted by thoſe, who pretend to give a true ſtate of human life, the firſt number near thrice as many as the uſual tables begin with? Or, could they be overlooked, if the computation began with 100,000, or a million?

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(11) Adding 2000 to the burials divided by 10, is, in effect, adding 20,000 to the whole. The increaſed column is 5 ſhort, which would ariſe from additional parts loſt, and make the full ſum. The laſt number in the Breſlau column ſhould not be 33, but 27. Dr. Brakenridge.

Further, ſhould the value of annuities ſink ſo precipitately, and cloſe ſo ſoon, will they be granted to perſons aged 95 for nothing, as the table of Mr. de Parcieux has it, in the ſupplement to Chambers's Dictionary? One would imagine thence, that thoſe aged 100, or more, ſhould have a premium to accept of them. And yet, what would be the conſequence, if the ſtate granted an annuity to 100,000 perſons, and the ſurvivors of them, to ſubſiſt intire to the death of the longeſt liver, and have it to pay 20 or 30 years beyond the utmoſt expectation repreſented in ſuch tables? It may here be obſerved, in reſpect to London more particularly, that the induction from this reſiduary part of life is well ſupported, ſince at 90, or later, few think of removing from town. Or, if it be inſiſted upon, that ſome do remove after that age, it will be allowed, that the burials of the ſubſequent years would have been higher, if they had all ſtaid and died there. It may ſeem quite impertinent to mention this to you, who have aſſured us, in general terms, that the burials (13) after 50 are leſs than they ought to be, allowing the accounts ſufficiently correct, and the numbers in later life not exaggerated, but rather the contrary.

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(12) Mr. Stonehouſe forms his account on 529,623, of which 181 lived to 100 years, and upwards; yet, beginning with 1000, cloſes his table at 95. It is needleſs to name many others in a general fault, to which the eaſy management of ſmall numbers is a temptation.

It will be found then of particular ſervice, that for thoſe of 100 years old, and upwards, we have the age of every ſingle death; and forming a table of them yearly decreaſing, and applying Dr. Halley's third rule of halving the tabular lives in any year, to diſcover the term expected, it will come out, that a life being, like one of the 242 aged 100, has an equal chance to live 2 years 3 quarters, or more (14); and, by his proceſs for finding the value from yearly chances, and at 5 per cent. it amounts to more than two years and a half purchaſe (15). Now, by your table, a life of 85 has not a better expectation; and following too cloſe upon your heels, Mr. Dodſon values an annuity of 1 l. for a life (16) of 88 but at 6 s. and 5 d. ready money. I ſhall not controvert this point; but deſire to know, who will grant ſuch annuities, or greater, for all that could be found of that age, or as many of them as ſhould be ſelected for nominees: I ſay nothing of the firſt number in his table. Doubtleſs you mean by 1 the firſt year of new-born children; and yet if he means the ſame, and values an annuity for a new-born child, whoſe equal chance of life is not 4 years, at 12.51 years purchaſe, it appears to me greatly over-rated, and I ſhould ſuſpect ſome fallacy in the method of computation.

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(13) Phil. Tranſ. Vol. XLIX. p. 175.

(14) As all the tables do or would reduce life to 0 before 100, how will they emerge again, to join conformably with theſe in term expected, or value? Yet theſe are realities, ſet in public view.

(15) Proceſs in MS. fol. 4.

(16) Phil. Tranſ. Vol. XLIX. p. 891.

Another thing I ſhall propoſe to your conſideration, is the forming a continual regiſter, if I may uſe that term, of the proportions of lives and deaths, by adding after each year, how many would have died out of 1000, or one out of how many, or both of them, in ſubſequent columns, with the differences, increaſing, or decreaſing, from year to year. If one only be uſed, I rather prefer the former, as it ſtrikes the eye, is a more natural repreſentation of increaſing mortality, and ſhews at once, if 1000 were to begin every ſingle year, how many of them would die in that year, in proportion to the lives and deaths of ſuch year in the table. The latter may be more agreeable to others, and is of ſingular uſe towards the end of life. I ſhall give a ſpecimen of both on your table for 3 decads, by which you will better perceive what I mean, and the uſes (17) that may be made of them.

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(17) Vide p. 61.

Year.	Lives.	Die.	Per mille.	One out of	Year.	Lives.	Die.	Per mille.	Diff.	One out of	Diff.	Year.	Lives.	Die.	Per mille.	One out of
11	406 —	4	9.85	101.5	21	368 —	4	10.87	— .12	92. —	— 1. —	31	325 —	5	15.38	65. —
12	402 —	4	9.95	100.5	22	364 —	4	10.99	— .12	91. —	— 1. —	32	320 —	5	15.63	64. —
13	398 —	4	10.05	99.5	23	360 —	4	11.11	— .12	90. —	— 1. —	33	315 —	5	15.87	63. —
14	394 —	4	10.15	98.5	24	356 —	4	11.24	— .13	89. —	— 1. —	34	310 —	5	16.13	62. —
15	390 —	4	10.26	97.5	25	352 —	4	11.36	— .12	88. —	— 1. —	35	305 —	6	19.67	50.833
16	386 —	3	7.77	128.66	26	348 —	4	11.49	— .13	87. —	— 1. —	36	299 —	6	20.07	49.833
17	383 —	4	10.44	95.75	27	344 —	4	11.63	— .14	86. —	— 1. —	37	293 —	5	17.06	58.6 —
18	379 —	4	10.55	94.75	28	340 —	5	14.71	— 3.08	68. —	— 18. —	38	288 —	6	20.83	48. —
19	375 —	3	8.00	125. —	29	335 —	5	14.93	— .22	67. —	— 1. —	39	282 —	5	17.73	56.4 —
20	372 —	4	10.75	93. —	30	330 —	5	15.15	— .22	66. —	— 1. —	40	277 —	6	21.66	46.166
	3885 —	38	97.77	1034.66		3501 —	43	123.48		824. —			3014 —	54	179.63	563.832

It is uſual with me, to note the yearly differences of mortality, which I could not do here, on account of the iregularity, but only in decad 21, &c. By the way, I muſt give a reaſon why I ſum up the column of lives, which I conſider as ſo many annual expoſures, and this as the total of the lives, each expoſed to the chance of mortality for one year; (i. e.) 406 in the firſt, 402 in the ſecond, &c. and 3885 in the ten years; and, upon the whole, 38 deaths.

Exp. Die. Exp. Die.
And thence 3885 : 38 :: 10,000 : 97.812; which laſt term is the proper ſtate or degree of mortality for that decad, and 9.78 a mean thereof, at an average.

It is generally acknowleged, that ſome one between the 10th and 20th is the healthieſt year, i. e. the year in which feweſt would die out of 1000, and the annual degree (18) of mortality ſhould increaſe (ſwifter or ſlower as it happens) from thence to the end of life. But how is ſuch year to be found among the irregularities of the firſt of theſe three decads? Or how ſhall we look upon 10. 11. 14. 15 as a due progreſſion in the ſecond? And if the numbers 16. 19. 20 do go on increaſing in the third, why does the degree of mortality go back to 17, then forward to 20, then back to 17 again, and forward to 21 per mille? in ſuch a manner, that one out of 49.8 ſhould die at the age of 36, and but one out of 58.6 at the age of 37; and again, one out of 48 ſhould die at the age of 38, and but one out of 56.4 at the age of 39? Is not this repreſenting the 37th year of life as a healthier than the 36th; and the 39th healthier than the 38th year of life?

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(18) A miſtaken inference from this, ſee p. 65. note (3c).

I am ſenſible you might ſay, this is owing to the promiſcuous changes of 5 and 6 in the deaths, for which you may have reaſons, though I cannot fathom them. With me it is not a matter peculiar to your table, but a certain conſequence of beginning with 1000 only: for having no changes, but what amount to an unit more, or an unit leſs, that is too great a leap at once, in ſuch ſmall numbers; when there might have been, in 500 and 600 deaths, room to expreſs duely the intermediate gradations of increaſe, or decreaſe, for every ſingle year.

The laſt thing I ſhall mention, is the term or expectation of life, ſhortning too ſwiftly, and then recoiling. I ſhall inſtance in your table, and Dr. Halley's too (and the ſame will be found in the reſt); and apply to them his rule above-mentioned, and the term, or probable expectation of life, will come out thus, for the ages following.

Year.	Lives.	Die.	Per mille.	Diff.	One out of	Diff.	Term of life.	Diff.
77	43—	6	139.5		7.16		4.12	
				—3.26		—1.00		—.25
78	37—	6	162.1		6.16		3.87	
								—.04
79	31—	5	161.2		6.2		3.83	
80	26—	4	153.8		6.5		4.—	
				—1.97		—.66		
81	22—	4	181.8		5.5		4.—	
82	18—	3	166.6		6.—		4.—	
								—.08
83	15—	2	133.3		7.5		3.75	
								—.50
84	13—	2	153.8		6.5		3.25	
				—.00		—.00		—.50
85	11—	2	181.8		5.5		2.75	

According to Dr. Brakenridge's table.

Year.	Lives.	Die.	Per mille.	Diff.	One out of	Diff.	Term of life.	Diff.
77	68—	10	147.0		6.8		4.—	
				—8.1		—.36		—.17
78	58—	9	155.1		6.44		3.83	
				—8.1		—.32		—.13
79	49—	8	163.2		6.12		3.70	
				—7.5		—.27		
80	41—	7	170.7		5.85		3.83	
				—5.7		—.19		
81	34—	6	176.4		5.66		4.50	
				—2.1		—.06		
82	28—	5	178.5		5.6		5.—	
83	23—	3	130.4		7.66		5.25	
84	20—	2	100.0		10.—		5.—	
85	18—	2	111.1		9.—		4.50	

According to Dr. Halley's table.

The column per mille, ſhould increaſe; and that one out of, ſhould decreaſe, with ſome ſort of regularity: I ſay no more of theſe, referring to what I have written above. My deſign here is to ſhew, that the term of life (19) decreaſing too ſwiftly, ſinks below the truth, and then ſtands ſtill, or increaſes, to become agreeable to the rule of nature, found in the courſe of ſubſequent years. Theſe irregularities and diſproportions might be avoided, by beginning with 43.00 and 68.00; or, it might be as ſatisfactory to many, to decreaſe the 4.12 years expected to 2.75, by proper intervals, or differences, greater above than below: an expedient not to be deſpiſed, when the capital points, from and to which, are previouſly ſettled on good authority. The term found by Dr. Halley's table is ſtill wider from the due courſe. I am apt to imagine, the conſciouſneſs of theſe difficulties induced you to ſtop ſhort, and perhaps you will wonder, that any one elſe would take the pains to ſurmount them.

But to return. You may poſſibly think it unreaſonable, that any body ſhould inſiſt upon ſuch a variety of ſcruples, in a caſe where the beſt means of information are too lax, and general; yet, I hope, will excuſe them, when aſſured, that my ſole reaſon for inſiſting upon them, is not a proneneſs to find faults, but an earneſt (you may call it an over-earneſt) deſire to make what we have better underſtood, and attain further means of ſuch knowlege, and ſtate them in the leaſt exceptionable manner: for till we get a more authentic account of life, or uſe what we have, without too much obſequiouſneſs to great names, we ſhall be as if blind and fettered. During ſuch prejudices and reſtraint, it is too early to compute values with minute preciſion, as ſome do, and wrangle about trifles, while they ſuffer firſt principles of greater moment to paſs quite unregarded.

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(19) I uſe this as the ſhorteſt form, meaning the equal chance, or probable expectation of life; as many ſurviving, as dying before ſuch period, among lives of the given age.

Whether my conjecture be right or not, Dr. Halley had reaſons why he left off at 84, as you may have for ending at 87; and the term of final direction is ſettled by another great maſter (20) at 86. But when many perſons outlive ſuch tables, and are moſt deſirous to purchaſe annuities, upon eaſy terms, for their lives, and have no rule at all left, it muſt be very acceptable, by whomſoever faithfully performed, to have a table beginning with the living births, formed upon 100,000 lives at leaſt, and carried on to the extremity, I ſhould almoſt ſay the utmoſt poſſibility of life, with the ſwift or ſlow increaſe of annual mortality, noted in a ſubſequent column, and in conſequence the term or expectation properly decreaſing, from the beſt life about 5 or 6, till the whole be exhauſted: and it would be a ſatisfaction to me, if, by ſuggeſting any hints, it may put abler hands at work, to bring it at laſt to ſuch a perfect ſtate, as I conceive, at preſent, in imagination only.

In proſpect of this, give me leave to obſerve, that the numbers in thoſe columns Per mille and One out of, may be of uſe for ſuch a purpoſe, as directors; and, by inverting the proportionals, be applied to find the deaths of lives given, from year to year (21): for as theſe may be previouſly digeſted, with greater regularity, and the number of deaths found, by multiplying the given lives by the former, or dividing them by the latter, a way is opened for conducting the work in larger numbers, and with great exactneſs: and I conceive, that ten of the beſt accounts of different places, each formed by proportion for 10,000 births, and all thrown together in the uſual ſections, if properly aided in the latter part of life by the London bills, might be ſufficient, at leaſt, would be much better than any thing we have; for tables of 1000, 1238, or 1400, are quite diſtanced here, having no lives at all of 100 years. And yet, who can believe that Dr. Halley's 34,000, if they were ſo many, much leſs Mr. Kerſeboom's 980,000, had none of 100 years and upwards; when the London bills afford about 8 in a year; and, upon the whole of 750,222 burials in the ſaid 30 years, have 242 of that age? Or, who can give a reaſon, if they had ſuch, why they conſtructed their tables on ſo ſmall a baſis, as wholly to exclude them?

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(20) Mr. De Moivre, pref. p. v. (edit. 1725.) tract, p. 10. 47. 76. 79. This ſtrikes off 14 years of Dr. Halley's table. Younger lives can hardly look ſo far forward, but old perſons ſee them at hand, and the value of all expectations is in proportion to their near approach.

It may be ſaid, the 242 perſons, dying above 100 years of age, did not ariſe from 750,222 births in town, but from a great many more brought in from the country after 20, and probably, upon the whole, from double that number of births. I ſhall only reply, let the number of births or perſons, natives or aliens, be what it will, from which the 242 ariſe, yet, in ſome definite number, ſuch and ſo many inſtances of longevity are found, of which the preſent tables take no notice; and though the fact is manifeſt enough, yet the abſurdity, in reſpect to practice and formation of tables, ſtill continues.

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(21) Theſe might be tried, or regulated, by proportions formed from other accounts, taking the deaths between 10 and 20 (or the moſt certain period) in each, for the two leading proportionals, and trying above and below for lives and deaths. Leſſer accounts of ſingle pariſhes will not do alone; for as aged lives are rare, one is forgotten before another happens, and in ſmall numbers they are not to be expected.

If we look back, we ſhall find the firſt ſketch, that of Capt. John (22) Graunt (alias Sir William (23) Petty) was formed upon 100 only, and ſuch a table carried the account to the 80th year, or upwards. Next were introduced thoſe of 1000, and extended the computation of life to between 84 and 100; tables formed upon 10,000 would advance to above 105; and upon 100,000, duly proportioned from the materials we have, might continue the account to 115 years, and upwards. If in the firſt ſketch, the ſuppoſed term of life was cloſed too ſoon, and it was an improvement to carry on an account of the gradual decay beyond the 90th year, why are we to reſt here, having additional obſervations made for more than 60 years, which furniſh materials for a further progreſs? If there is room, and good foundation to advance but 20 years beyond the compaſs of the preſent tables, ſhould not this be done? And will it not make a conſiderable, yet neceſſary, alteration in all computed values, upon annuities to be granted to perſons in the latter part of life?

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(22) Graunt's Obſervations, 5th edit. p. 84.
(23) Phil. Tranſ. N° 196.

I have not ſeen Mr. Smart's tract on the London bills, (when and how was it publiſhed?) nor the collections of Mr. Dupre, publiſhed by Mr. Buffon, ſave only through the medium of Mr. Kerſeboom's proportions (24), beginning with 1000, not births, but children of 6 months old, or upwards (25); which makes a conſiderable difference in reſpect to age given, and yearly deaths, through the whole table; and I almoſt envy thoſe, who have the deſirable uſe of choice originals. The accounts of Dr. Newman are, I ſuppoſe, preſerved by your ſociety; and there is a ſtate of the exchequer annuitants (26) often mentioned, but not publiſhed, by Mr. Lee. Neverthelefs, theſe laſt being of divers ages (if the particular age of each perſon at entry and death be not known, though the groſs numbers yearly dying may), as it was too great a preſumption to aſſert, that they began all at the beſt ſtage (27) of life, and were ſo nicely choſen (28), that the duration of 35 years was a thing extraordinary; ſo it would be a blameable credulity to admit theſe points for truths, when we continually ſee how many are reſolved to chuſe their own lives, or thoſe of their children or favourites, even when they are receded 10 or 20 years from that part of life, which had the largeſt expectation. Whether it was this matter better conſidered,

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(24) Phil. Tranſ. 1753. p. 239.

(25) This was done, to compare it rightly with Dr. Halley's, which Mr. K. therefore knew was not from 1000 births.

(26) Lee's Eſſay, 252, 253. " This," he ſays, " is the " beſt guide of all." Lee's Val. Annuit. p. 47. 51.

(27) Eſſay 252.

(28) Eſſay 253.

ſidered, or whatever elſe changed that author's ſentiments, yet changed they were; for, in 1737, he accounted a life of 10 years beſt, and equal to a term of 28 years, and no more; [Lee's Eſſay, p. 231. 253.] and yet, ſince, in (his Valuation of Annuities, 2d edit. p. 96.) 1754, he has computed the ſame kind of life, as equal to a term of 35 years, notwithſtanding all the allowances pleaded for in his Eſſay (29); in full conſideration of which, he was afraid of overdoing the matter. And yet, if he would have given us the true reſult of the London bills, according to his own ſtate, and reckoning with exactneſs, which he calls to the extremity, a life of 10 years would be found equal to a term of 34.94 years; but one of 4 years old equal to a term of 38.20 years; above 3 years better than his beſt life.

I ſhould not have mentioned this, but to obſerve, that it is natural enough, when the expected term of life is taken to the uttermoſt, to make ſome allowance. And yet, if allowance is to be made, it ſhould not be by an arbitrary and falſe repreſentation of life throughout a whole table (30), but left to the diſcretion of parties concerned, and to be made in proportion to

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(29) Page 231.

(30) Mr. Lee rightly concluding, that the degree of mortality ought to increaſe from the year of greateſt expectation to the end of life, erroneouſly inferred from thence, that the deducend, or part of the term of life waſting in each decad, ſhould be leaſt at firſt, and greater afterwards; and ſo apportioned it, as long as he could go on, contrary to all other tables, and even to the courſe that would ariſe from his own table of the London bills. Lee's Eſſay, p. 459. Table II.

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the term found by ſuch table; and that, in general, about $\frac{1}{10}$ of the term deducted, in any part of life, would not be unreaſonable, when the account is carried on to the full extent of human life, eſpecially if ſuch ſuppoſed duration is taken as a rule for price; for reaſons well known to you, by comparing the reſpective (31) value of preſent and future years at any rate of intereſt (32), and obſerving, that a gain of time in the latter, would not be equal in value to the loſs of like time in the former.

After many degrading compariſons above-mentioned, to the diſhonour of our capital, it may counterballance them, to hear what this advocate has urged for its healthfulneſs and longevity, on his own experience. (Lee's Eſſay, p. 252, 253.) I do not intend to diſparage it, by mentioning a place I have known above 40 years, never famed for ſalubrity, and yet has produced as many (may I ſay more?) perſons of 90, and upwards, than London, as would appear on a fair proportion, formed on the burials of each, by a method too long to be laid before you at preſent; and I am ready to oppoſe this, as I did to the former complaints, ſo to theſe boaſtings, being both alike ill-grounded and unreaſonable.

As my objections extend to all the accounts I have met with (perhaps I ſhould except Mr. Dupre's, if I could ſee it intire), you will naturally expect what

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(31) This, I find, has been noted by H. B. in obſervation on Lee's Eſſay, 1739. p. 18. 21. 33.

(32) This makes hazard of time and hazard of value unequal in courſe.

I have to offer more perfect in their ſtead. The ground-work I preſent would lie in a nut-ſhell (33); the edifice would ſupply matter for ſuch another letter: and, I think, you will be glad to have a little reſpite, to conſider of this, and judge how far ſome facts herein ſet forth ought to be regarded, without which, I preſume to affirm, all computations of value will be found inconſiſtent, and very faulty in ſome other parts of life.

Upon ſuch an occaſion as this, I hope you will excuſe the intruſion and tedioufneſs of a new viſitor, who aims at further improvements on this ſubject, both in matter and form. I willingly ſubmit the reſult of not a little time and pains to your ſuperior judgment, and beg you will either frankly pronounce it time miſpent, and labour in vain, or elſe, by your kind directions, enable me (if the ardor of preſent inclination ſhould continue) to improve and finiſh a ſcheme, perfect enough to bear public view. I can only ſay, I ſhould endeavour to keep clear of the objections made to other tables, and to ſupport whatever I advance by real facts, or very ſtrong probabilities, and to make the whole conſiſtent and uniform. I do not enter upon any computation of values, nor prefer either of the two methods (34) for finding them; let the facts, upon which they depend, be better aſcertained. In order to this, the ſole object of my preſent view is, to ſtate, with all exactneſs, the term and period of human life, being ambitious ſo to execute this underpart of the work, as may deſerve yours, or general approbation.

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(33) See p. 69. letters (*g*), (*h*).

(34) One propoſed by Dr. Halley, and approved by Mr. De Moivre, the other by Mr. Lee. Richards's Annuities, p. 1. Lee's Valuation, p. 2.

Having delivered this meſſage, you may form in idea the aukward ruſticity of a ſtranger, introduced the firſt time to your preſence, who hardly knows how, or when it is fit, to make his bow, and withdraw.

I am,

Reverend Sir,

Your great admirer,

and moſt reſpectful

humble ſervant,

T. W.

N. B. In what follows, I am obliged to number 1 year beginning, for the ſake of others, which I uſually mark 0, and the reſt 10, 20, 30, &c.

POSTSCRIPT.

Year beginning.	Deaths.		Deaths.		Deaths.		Deaths.		Deaths.		Deaths:
1	579½	495	581	469	347	122 leſs.	806	474	667	459	466½
11	46	39	62	50	61	11	93	55 +	61	42 +	48½
21	68	58	68	55	69	14	114	67	130	90 +	78½
31	81	69	86½	70	87	17	138	81	96 (1)	66	73½
41	84	72	100	81	101	20	130 (1)	77 +	105	72	74½
51	100½	86	103½	84	103	19	148	87	114	78	82½
61	103	88	100½	81	101	20	135	79	119	82 +	80½
71	85	73	99	80	97	17	81	48 +	97	67 +	57½
81	19	16	30	24	27	3	44	26 +	58	40 +	33
91	4½	4	7½	6	7 (3)	1	11	6	5	3	4½
101	0	0	0	0	0	0	0	0	1	1 + (2)	0½
	1170½	1000	1238	1000	1000	122 more.	1700	1000	1453	1000	1000
	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(b)	(i)	(k)	(l)

Thrown back upon theſe decads

(1) The caſe of a leſſer decad between two greater, is not unuſual. I find the like in Dupre, Kerſeboom, Edenborough, and Norwich, not all in the ſame decad; divers accounts rectify one another; a ſingle one may be made diſcreetly ſmooth.

(2) At Ely-Trinity, one now (1760) living aged 103, and one died within memory at 106, and upwards, may be ſet at about one in 3000 dying at 100 years old, or upwards; but who will live to take account of them?

(3) If (e) were taken unreformed, Breſlau would, after 10, be more unhealthy than (b) or (k).

(*a*) The reſult of Dr. Halley's proportionals, (Lowthorp, Vol. III. p. 670.) filled up from year to year, to the 100th year. As theſe come ſo near 1174 (ſtated p. 669. as the annual deaths), it ſhould ſeem as if the firſt attempt was made that way, to find how many of ſuch deaths would fall in each decad.

(*b*) Exhibits the ſame for 1000 births.

(*c*) By taking in the 238 that were ſuppreſſed, ſtating the firſt 6 years, according to the preliminary diſcourſe, and continuing the account according to the table; only ſetting back half a year, or poſtponing 4 deaths to the 2d decad, [and ſo half the laſt year's deaths from every decad to the following] and this gives the 1238 intire.

(*d*) Exhibits the laſt preceding, computed for 1000 births, and as the table ought to be from thoſe data, but yet ill proportioned.

(*e*) Shews the decrements, or deaths, according to Dr. Halley's table, beginning with 1000 perſons, and exhibiting a ſuppoſed body of coexiſting people, in all 34,000. The firſt decad, ſo much below the truth, from 1000 births only, that it ſwells the 9 decads following with 122 deaths more than ſhould be, as is repreſented in (*f*).

(*g*) The deaths of 1700 perſons in the pariſh of Ely-Trinity.

(*h*) The ſame proportional for 1000 births.

(*i*) and (*k*) The like for All-Saints in Northampton.

(*l*) The halves of the ſums of (*h*) and (*k*) for 1000 births. For, by comparing (*d*) and (*e*), you will ſee, that the firſt decad being leſs than it ſhould be, (i. e.) ſo much ſhort of 469, throws back 122 deaths upon the other decads of life. And if this were 469, as it ought, it would fall between (*h*) and (*k*), Ely-Trinity and All-Saints in Northampton. As this happens in the firſt decad, I have joined both accounts, and halved them; and the reſult of it is ſtated at (*l*); which will be found as near (*d*) as can well be expected, in accounts of different places; and with this I ſhall conclude this Poſtſcript.

XII. *Ex-*

XII. *Experiments on checking the too luxuriant Growth of Fruit-Trees, tending to diſpoſe them to produce Fruit: In a Letter to Mr.* Peter Collinſon, *F. R. S. from* Keane Fitzgerald, *Eſq; F. R. S.*

Dear Sir,

Read Feb. 12, 1761.

WHEN you did me the favour of calling on me at Fulham, in autumn 1759, I ſhewed you ſome experiments I had made, in order to check the too luxuriant growth of young trees; which I promiſed to give you an account of, if they ſhould ſucceed according to my expectation.

I had obſerved a method taken to bring young trees to bear, when planted in too rich a ſoil, by cutting away part of the bark from ſome of the main branches. This method, as I am informed, has brought them ſoon to bear plentifully; but leaves an ugly wound, the wood continuing bare, and apt to rot in that part.

I had ſome young plumb and cherry trees planted againſt a north pale, in a very rich ſoil. The plumb-trees had, in three years, ſhot forth the extremities of their branches to 15 or 16 feet diſtance, and had quite covered and overtopped the pale. As the cutting away of any of theſe branches would make the reſt ſhoot the ſtronger, I made the following experiments, about the middle of Auguſt 1758.

I made a circular inciſion on the main arms of an Orleans plumb-tree, near the ſtem, quite through the bark, where it was ſmooth, and free from knots. About 3 or 4 inches higher, I made another inciſion, in the ſame manner; then making an inciſion lengthways, from the upper to the under circumciſion, I ſeparated the bark intirely from the intermediate wood, covering it, and alſo the bare part of the wood, to keep the air from the wound; and letting them remain ſo for about a quarter of an hour, when the wound began to bleed, I replaced the bark as exactly as I could, and bound it round pretty tightly with baſs, ſo as to cover the wound intirely, and alſo about half an inch above and below the circumciſions.

I treated the intire ſtem of a duke cherry-tree in the ſame manner, about 10 inches from the ground, and below all its branches. Alſo ſeveral branches of a morelli cherry-tree; and the main arms of two perdrigon plumb-trees. Theſe two laſt were old trees, which had been cut to the ground about four years before, and had ſhot forth very luxuriant branches, but had not ſince borne any fruit.

In about a month's time, the bark of theſe began to ſwell, both above and below the binding; when I unbound each of them, and found the ſeveral parts, that had been replaced, to be all fairly healed, except one, which was on the main arm of the perdrigon plumb-tree, part of which was healed, and about an inch in breadth of the bark, on one ſide of the longitudinal inciſion, remained looſe, and afterwards dropped off. I bound them all again lightly with baſs, and let them remain ſo, until the beginning of the ſummer following; when I took off the binding intirely, and found them all healthy, and flouriſhing.

Each of theſe trees bore plentifully that ſeaſon, though, in general, reckoned a bad year for fruit.

This induced me, in the beginning of Auguſt 1759, to make the like experiments on ſeveral other young trees; ſome, that had not yet borne any fruit, and others, that had borne but a ſmall quantity; particularly, two young pear-trees, that never yet had any bloom. I treated the main arms of one of theſe in the manner already deſcribed, and alſo ſeveral of the branches, that grew on theſe arms; likewiſe one of the arms of the other pear-tree. The firſt of theſe bore a ſurprizing quantity of fruit laſt ſummer; and the circumciſed arm of the other bore a moderate quantity, though no other part of the tree had any appearance of bloom.

I made alſo the following experiments, on two branches of different young apple-trees, as nearly of the ſame ſize as I could find. I cut off the bark of theſe, as exactly as I could by a gage; changing them, and putting the bark of the branch of one tree on the branch of the other. I find, by the minutes I took, that a ſmall ſlip of wood came off with the bark of one, and the bark of the other had a leaf-bud on it; which branch had alſo two apples growing on it. The bark of each of theſe healed perfectly, and the apples remained on, and ripened with the reſt: the leaf-bud puſhed forth leaves, and both the branches bore ſo very plentifully the laſt ſummer, that one broke down with its load; and the other would alſo, probably, have ſuffered the ſame fate, but that I had it ſupported. Theſe were both nonpareil apple-trees, planted in aſparagus beds.

I changed the barks of the branches of a peach and a nectarine tree; that, which was placed on the peach-tree, healed perfectly, and the branch produced a quantity of bloom laſt ſeaſon; but the bloom of the whole tree, as well as of ſeveral others againſt the ſame wall, was intirely blaſted. The gardener cut off the branch of the nectarine, when he was pruning, and nailing the trees, as he did of ſeveral others, on which I had made experiments of the ſame kind; againſt which he declared his opinion ſtrongly, at the time of making; and ſaid, he was ſure the branches would all die, and the wall be quite bare in theſe parts; which, I ſuppoſe, he imagined would be a reflection on his ſkill in pruning and nailing a tree.

About the beginning of November laſt, I cut off one of the arms of the perdrigon plumb-tree, which had the experiment made on it in 1758, to examine what effect it had on the wood; to which, I found the bark between the circumciſions more firmly united, than in any other part. There was a dark vein, which ran through the wood in that part, which appeared of a harder texture than the reſt of the branch.

On examining the minutes I had taken from time to time, of the obſervations I had made on theſe experiments, which I imagined I had been very exact in, I find I had omitted noting down any relative to the effect they had on the growth of the circumciſed branches. I did not compare them by meaſure with other branches; but as far as I can ſpeak by recollection, it has retarded the growth. I can be almoſt poſitive, that the cherry-tree, mentioned in the ſecond experiment, the trunk of which had been circumciſed

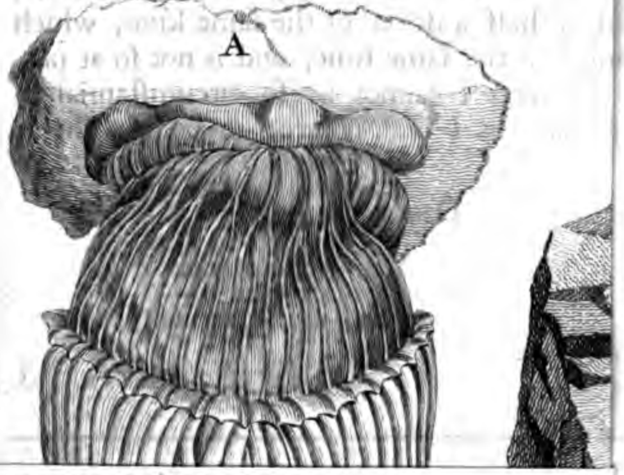
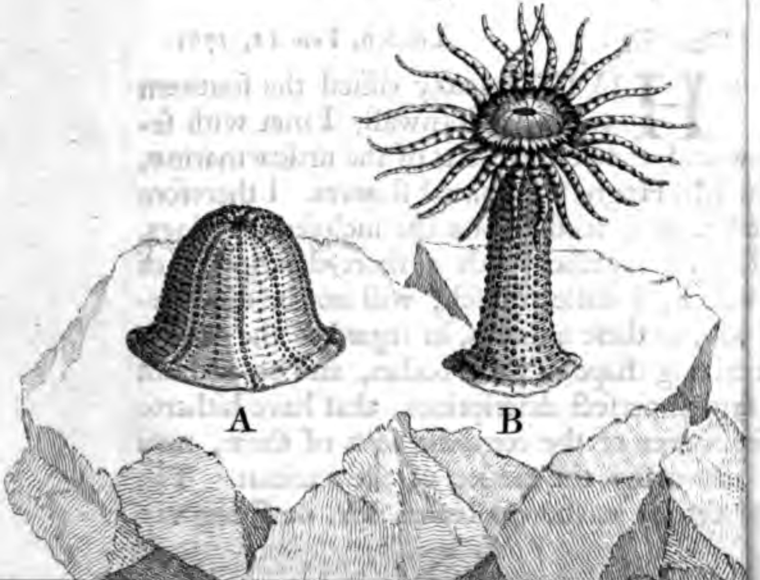


Fig. 4.



circumciſed below all its branches, was, at the time, the largeſt of half a dozen of the ſame kind, which were planted at the ſame time, and is not ſo at preſent. I am ſorry I cannot be ſo circumſtantial in this particular, as I endeavoured to be in others; and am,

Dear Sir,

Your affectionate

humble ſervant,

Poland-Street,
Jan. 19, 1761.

Keane Fitzgerald.

XIII. *An Account of the* Urtica Marina: *In a Letter to Mr.* Peter Collinſon, *F. R. S. from* Joſeph Gaertner, *M. D.*

Dear Sir, London, Feb. 12, 1761.

Read Feb. 12, 1761.

HAVING lately viſited the ſouthern coaſts of Cornwall, I met with ſeveral new and undeſcribed ſorts of the urticæ marinæ, called by Mr. Hughs the animal flowers. I therefore take the liberty of ſending you the incloſed drawings, [*Vide Tab.* I.] together with a ſhort deſcription of them, which, I flatter myſelf, will not be diſagreeable to you, as theſe animals, in regard of the various and ſurprizing ſhape of their bodies, and on account of the few imperfect deſcriptions, that have hitherto been given even of the common ſorts of them, may not be unworthy the notice of the curious. The name of urtica, as the celebrated Mr. de Reaumur,

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in a memoir relating to that ſubject *, juſtly obſerves, has been very improperly applied to this kind of animals; for it is certain, that not a ſingle ſpecies of them is poſſeſſed of that ſtinging quality like a nettle (which the antients aſcribed to them), and that only their tentacula feel rough and clammy, when touched with the finger. Even this roughneſs is not perceptible, but when the animal attempts to lay hold of the finger: it then throws out of the whole ſurface of the feeler a number of extremely minute ſuckers, which, ſticking faſt to the ſmall protuberances of the ſkin, produce the ſenſation of a roughneſs, which is ſo far from being painful, that it even cannot be called diſagreeable.

The proper genus, which theſe ſea-nettles belong to, is that of the hydra of Linnæus, commonly called the polype. This will evidently appear, from the following characters: firſt, from the gelatinous ſubſtance, of which this whole tribe of animals conſiſts: ſecondly, from their having only one opening in their bodies, which gives a paſſage to the food, as well as to the excrements, of the animal: and laſtly, from a ſet of feelers, which ſurround this opening, and ſerve theſe creatures for claws, to catch their prey with, and convey it to their mouths. As the ſea-nettles agree perfectly in thoſe general characters with the hydra, ſo do they alſo anſwer to many of its leſs eſſential, or merely accidental qualities: they live, for inſtance, conſtantly in the water, in which

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* Du mouvement progreſſif et de quelques autres mouvemens de diverſes eſpêces de coquillage, orties, et etoiles de mer. Memoires de l'Academie Royale des Sciences, 1710. p. 439, &c.

they never ſwim, but always adhere to ſome fixed body in it; and when they change their place, moſt of them crawl along ſo very ſlowly, that their progreſſive motions cannot be perceived with the eye to. This may be added, that they likewiſe bring forth their young ones alive, and that they grow again, after conſiderable parts of their bodies have been cut off: all which proves ſtill farther, that theſe animal flowers, or ſea-nettles, are of the ſame nature, have the ſame characters, and do conſequently belong to the ſame genus, with the hydra. The polypes in general may be divided into two claſſes, the one containing thoſe polypes, that cannot conceal their feelers, though ever ſo much irritated; and the other, thoſe, that, at the leaſt irritation, contract themſelves, draw in their feelers, and frequently hide them under a membranaceous cover made for that purpoſe. The firſt claſs, on account of the ſmall number of ſpecies belonging to it, needs no ſubdiviſions; but to diſtinguiſh properly the ſeveral ſorts of the ſecond claſs, it is neceſſary to divide it according to the various poſition of the feelers, which are inſerted either in the membranaceous cover itſelf, or into a flower-like production of the body, or laſtly, in the very top part, or the diſk of the polype: hence ariſe the three following ſubdiviſions of the ſecond claſs: 1. Hydra calyciflora. 2. Hydra corolliflora; and laſtly, Hydra diſciflora. The reaſon for which appellations will be farther explained, in the deſcriptions I am now going to give of every ſort in particular.

The firſt claſs conſiſts but of a ſingle ſort, whoſe ſpecifical character may be thus expreſſed:

Hydra tentaculis denudatis, numeroſiſſimis; corpore longitudinaliter ſulcato.

The natural ſize of this animal, grown to its full age, is repreſented in the firſt figure, lit. A, ſhows the animal ſuſpended in the air, and lit. B, is the ſame whilſt under water. The body of this polype is of a light cheſnut colour, and feels perfectly ſmooth, though it be lengthways ſulcated by a number of ſulci, that are frequently divided into three ſmaller ones, and are continued into the dentated margin, that ſurrounds the upper periphery of the body, juſt beneath the inſertion of the feelers. Theſe feelers, riſing from the diſk of the polype, are, according to the age of the animal, between 120 and 200 in number; they exceed the body, when expanded, by more than an inch in length, and are of a beautiful ſea-green colour, except towards their extremities, which are coloured with a lively red, like that of the roſe. The diſk is of the ſame brown colour with the reſt of the body, and contains in its center the mouth of the animal, which is an aperture of various ſhape and diameter.

The two varieties of this ſpecies, which I met with, differ but little from the already deſcribed animal. The feelers of the one, inſtead of being green, are throughout of a red colour, like that of the mahogony wood. The other variety has pale aſh-coloured feelers, marked with a ſmall white line running along their back; its body is of the ſame cheſnut colour with that of the firſt ſpecies; but the ſulci are not divided, nor has it a dentated margin ſurrounding its upper periphery.

I doubt whether theſe animals have yet been taken notice of by the curious, though they are very frequent upon the ſea-coaſts. A rough ſketch of an animal ſomewhat like this is to be found in Aldrovandus *, with the inſcription, Urtica marina ſaxo innata. But as neither he, nor Johnſton †, who copied the figure from Aldrovandus, gives any farther explanation of it, it is incertain what ſpecies of urtica the ſaid figure repreſents.

The polype belonging to the ſecond claſs, concealing their feelers when irritated, are the following:

Hydra calyciflora, tentaculis retractilibus variegatis, corpore verrucoſo.

The ſecond figure repreſents a polype of this ſort. From its ſmall baſis riſes a cylindric ſtalk, which ſupports the roundiſh body of the animal, from whence afterwards the calyx, being a continued membrane of the body, draws its origin. The ſtalk, or the pedunculus of the polype, is quite ſmooth, and its colour inclines towards the carnation. The outſide of the calyx, and the body of this animal, are marked with a number of ſmall white protuberances, reſembling warts, to which fragments of ſhells, ſand-grains, &c. adhere, and hide the beautiful colour of theſe parts, which, from that of carnation, is inſenſibly changed towards the border of the calyx, firſt into purple, then violet, and at laſt into a dark brown. The inſide of the calyx is covered with the feelers, that grow in ſeveral ranges upon it: they differ conſiderably in length; thoſe that are near the edge of the calyx being but ſmall papillæ, in proportion to thoſe, that ſurround the diſk, or the central part of the body. (Vide fig. 2. lit. C.) They are almoſt tranſparent; and ſome of them are of a pale aſh colour, with brown ſpots; others, on the contrary, are of a cheſnut colour, marked with white ſpots. The diſk is formed like a ſtar, which, according to the figure, that is traced out by the innermoſt row of the feelers, conſiſts of many angles. The colour of this part of the body is a beautiful mixture of brown, yellow, aſh-colour, and white, which together form variegated rays, that from the center, or the mouth of the animal, are ſpread over the whole ſurface of the diſk.

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* Aldrov. de Zoophyt. lib. iv. p. 568.

† Johnſt. Exang. Tab. XVIII.

This polype contracting itſelf, (vide fig. 2. lit. B) changes its body into an irregular hemiſphere, which is ſo covered with the ſeveral extraneous bodies that ſtick to it, that it is extremely difficult to know the animal in this ſtate, and to diſcern it from the rubbiſh, that commonly ſurrounds it.

Theſe animals are frequently found in the pools about the Mount's-Bay. It is rare to meet with a ſingle one in a place, there being moſt commonly four or five of them living ſo near together in the ſame fiſſure of the rock, which they conſtantly inhabit, that their expanded calyces form a row of flowers like bodies, that ſeem to grow upon the cliffs under water.

The ſecond ſpecies, is the

Hydra corolliflora, tentaculis retractilibus frondoſis.

This animal, in its contracted ſtate (vide fig. 3. lit. A), has more the appearance of a caterpillar, than of a polype. Its body is covered with a duſky white ſkin, in which a large opening appears at the thicker extremity of the body, and at the oppoſite end of it are 5 ſmall denticles, that ſurround a cavity placed in their middle. The ſurface of this cylindrical body is marked with ſix double rows of perforated knots, which the animal can transform into as many legs, if occaſion requires, by extending each tuberculum into a ſmall tranſparent cylinder, whoſe extremity, like that of the ſuckers of the ſtar-fiſh, ſticks faſt to every thing, which the animal gets hold of, and conſequently ſerves it for an inſtrument, not only to fix its body with, but alſo to puſh it forward, by the help of many of theſe ſuckers, that are formed of the ſeveral knots of different rows. The head of the polype (vide fig. 3. lit. B) coming out of the above-mentioned opening in the ſkin, is of an oval, and ſometimes of an hemiſpherical figure, ſomewhat like the corolla of an aſarum, but much larger in ſize. It is quite hollow within, and conſiſts of a dark brown, yet almoſt tranſparent membrane, which, after having formed the head, produces the feelers, that ſurround the large aperture at the top of it. Theſe feelers are eight or ten in number, and of the ſame ſubſtance and colour with the head; they are divided into ſeveral branches, to which, as well as to the principal ſtems, many cluſters of very minute papillæ adhere, which make them exactly reſemble ſmall branches of trees covered with their leaves. Theſe leaves, or papillæ, not only contribute to the beauty of the feelers, being of a pale yellow, mixed with a ſhining white like ſilver, but they alſo render the feelers more uſeful to the animal, in filling up the interſtices between them, through which ſmaller inſects elſe might paſs, without being perceived by the animal, whoſe natural food they are.

This polype ſeems to live at the bottom of the ſea, diſtant from the land. I met but once with it upon the ſhore, between Penzance and Newland, where it was thrown up by the ſea, incloſed in a large hollow root of the fucus palmatus.

The third ſpecies, is the

Hydra diſciflora, tentaculis retractilibus ſubdiaphanis; corpore cylindrico, miliaribus glandulis longitudinaliter ſtriato.

A polype of this ſort is repreſented in the fourth figure. Its body, when extended, is of a cylindrical figure, and conſtantly marked with ſome rows of ſmall knots, or glandulæ, that are placed in ſtrait lines from the top to the baſis of this cylindrical ſtalk. Each row is compoſed of three files of glandulæ, of which the middle one is remarkably bigger than the two others; their number is uncertain, yet I never met with leſs than eight rows in an animal grown to its full age. The colour of the ſtalk near its baſis is a pale red, and the reſt is of a yellow, mixed with a grey aſh-colour. The glandulæ are almoſt of the ſame colour with the body, except thoſe of the middle file of each row, which I conſtantly found to be white. Out of the top part, or the diſk of the polype, grow the feelers, from eighteen to thirty-ſix in number; they are of a half-tranſparent ſubſtance, and of a whitiſh colour, variegated only at the upper part of the feeler, like the back of ſome ſnakes, with ſeveral croſs-lines, and brown ſpots of an irregular figure. The diſk of this polype is always convex, and chiefly of an orange colour, except towards its periphery, which is marked with many dark brown ſpots, that ſurround the inſertion of the feelers.

At the leaſt irritation, this animal contracts its body, and changes the cylindrical figure of it into a conoidal one. (Vide fig. 4. lit. A.)

The fiſſures of the rocks in the ſea are the only place, where I met with this ſort of polypes, which is not common upon the coaſts of Cornwall.

Of this ſpecies I found two varieties. The top parts of the one are in ſhape and colour much the ſame with thoſe of the already deſcribed animal; the ſtalk only is of a deep green colour. The ſecond variety has likewiſe a green ſtalk; but its feelers are not variegated, being throughout of a pale and tranſparent red colour.

The animal flowers of Mr. Hughes *, and the ſea-nettle, with a ſhagreen ſkin (Ortie a peau chagrinée), of Mr. de Reaumur †, may, perhaps, belong to this ſubdiviſion.

The laſt ſpecies of theſe polypes I have to propoſe, is the

Hydra diſciflora, tentaculis retractilibus, extimo diſci margine tuberculato. (Vide fig. 5. lit. A et B.)

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* Philoſ. Tranſact. Vol. XLII. p. 590.

† Mem. de l'Acad. Roy. des Sciences, Tab. X. fig. 21.

I only mention this ſpecies, to determine its ſpecifical character, which has not yet been given by any of the authors, that have already taken notice of this animal *. The colour of its body is always red in the ſummer, but changes into a duſky green, or brown, towards the latter end of autumn. The outſide of it is quite ſmooth, ſome few animals of this ſort excepted, which are marked, like the firſt ſpecies of this claſs, with ſmall protuberances, to which ſeveral extraneous bodies likewiſe adhere. The feelers are conſtantly inſerted into the diſk of the polype, but they are of various colours, viz. red, blue, white, and ſometimes even variegated. Between theſe feelers and the membranaceous cover of the animal, is a row of ſmall hemiſpherical tubercula, which, though they vary in colour as much as the feelers, yet are conſtantly found to be placed upon the edge or periphery of the diſk, and conſequently afford, together with the inſertion of the feelers, a certain mark, by which this animal, ſo variable in its colour and ſhape, may be at all times known and diſtinguiſhed from any other ſort belonging to this tribe.

This is what occurred to me, on the figure and external parts of theſe animals. I could add a deſcription of their internal ſtructure, and ſome obſervations on the manner of their propagation; but, as I have already tranſgreſſed the limits of a letter, I ſhall defer

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* Bellon. de Aquat. lib. ii. p. 342. Rondelet. de Piſc. lib. xvii. cap. 12 et 14. Geſner. Hiſt. Anim. p. 1037. &c. Aldrov. de Zoophyt. lib. iv. p. 567. Johnſt. Exang. Tab. XVIII. De Reaumur, lib. c. Tab. X. fig. 22. 24.

enlarging on this ſubject, till another time; and conclude with aſſuring you of the moſt perfect eſteem, with which I have the honour to be,

Dear Sir,

Your moſt obedient,
humble ſervant,

Joſeph Gaertner, M. D.

XIV. *A Catalogue of the* Fifty Plants *from* Chelſea Garden, *preſented to the* Royal Society *by the worſhipful Company of Apothecaries, for the Year* 1760, *purſuant to the Direction of Sir* Hans Sloane, *Baronet, Med. Reg. & Soc. Reg. nuper Præſes, by* John Wilmer, *M. D. clariſſ. Societatis Pharmaceut.* Lond. *Socius, Hort.* Chelſean. *Præfectus & Prælector. Botanic.*

Read Feb. 19, 1761.

1901 ALCEA vulgaris major, flore ex rubro roſeo. C. B. 316.
1902 Andromeda pedunculis aggregatis, corollis cylindricis, foliis alternis ovatis integerrimis. Linn. Spec. Plant. 293.
1903 Ariſtolochia longa vera. C. B. 107. Offic. 47.
1904 Aſclepias foliis revolutis linearibus verticillatis, caule erecto. Linn. Sp. 217.
1905 Aſphodelus foliis planis, caule ramoſo, floribus ſparſis. Dict. Hort. Icon.

1906

1906 Baſteria foliis ovatis oppoſitis, floribus lateralibus, caule fruticoſo ramoſo. Dict. Hort.

1907 Camphoroſma foliis hirſutis linearibus. Amœnit. Acad. Sp. 392.
Camphorata hirſuta. C. B. P. 486.

1908 Cardiaca. J. B. 3. 56.
Marrubium Cardiaca dictum. C. B. 230. Offic. 104.

1909 Cedrus folio cupreſſi, media, majoribus baccis. C. B. P. 487.
Cedrus Phœnicia, altera Plinii et Theophraſti. Lobel. 221.

1910 Ciſtus arboreſcens foliis ovato lanceolatis acuminatis trinerviis ſeſſilibus utrinque villoſis.

1911 Coronilla fruticoſa ſtipulis ſubrotundis. Linn. Sp. Pl. 743.

1912 Coronopus ſylveſtris hirſutior. C. B. 190. Offic. 147.

1913 Damaſonium ſtellatum. Lugd. 1058.
Plantago aquatica ſtellata. C. B. 190.

1914 Filix mas non ramoſa dentata. C. B. 358.

1915 Galeopſis paluſtris Betonicæ folio. Tourn. Panax Coloni Officin. 341.

1916 Gentiana paluſtris anguſtifolia. C. B. 188.
Pneumonanthe. Ger. 355.

1917 Gundelia foliis pinnatifidis ſpinoſis, capite araneoſa lanugine obſita. Miller's Icons.

1918 Helleborine latifolia montana. C. B. P. 186.

1919 Hibiſcus foliis inferioribus trilobis, ſummis quinque partitis obtuſis crenatis, calycibus inflatis. Dict. Hort.

1920 Kalmia foliis lanceolatis corymbis lateralibus. Linn. Gen. nov. 1079.

1921 Lycium foliis cordato-ovatis ſeſſilibus oppoſitis perennantibus, ſpinis craſſis bigeminis, floribus confertis. Dict. Hort.

1922 Lycopodium Sabinæ facie. Fl. Jenenſ. 328.
Muſcus clavatus foliis cupreſſi. C. B. 360.

1923 Lyguſticum quod Seſeli Offic. C. B. 162. Offic. 452.
Siler montanum major. Mor. Umb. 7. 8.

1924 Lyſimachia ſpecioſa quibuſdam onagra dicta ſiliquoſa. J. B. 2. 906.
Chamenerion flore Delphinii. Park. Par. 270.

1925 Malva caule erecto herbaceo, foliis lobatis, ſpicis ſecundis axillaribus. Linn. Sp. 688.

1926 Magnolia foliis ovato-lanceolatis ſubtus glaucis annuis. Dict. Hort.

1927 Meſpilus Cretica folio circinato et quaſi cordiformi. T. Cor. 43.
Chamæceraſus Idæa, Alpini Exotic. 5.

1928 Meſpilus folio ſubrotundo, fructo rubro. Tourn. 642.

1929 Milium ſemine luteo vel albo. C. B. 26. Offic. 317.

1930 Morus foliis palmatis, fructibus hiſpidis. Linn. Sp. Pl. 986.
Morus ſativa foliis urticæ mortuæ, cortice papyrifera. Kempf. Amæn. 471.

1931 Narciſſus ſpatha uniflora nectario maximo limbo fimbriato petalo longiore.

1932 Narciſſus ſpatha uniflora nectarii limbo campanulato erecto petalo æquali. Linn. Sp. 289.

1933 Naſturtium ſupinum capſulis verrucoſis. Ray Method. emendata, 98.
Coronopus Ruellii. Ger. 346. Offic. 343.

1934 Ornithopodium radice nodoſa. Park. 1093.

1935 Panicum Germanicum ſive panicula minore. C. B. 27. Offic. 343.

1936 Pimpinella ſanguiſorba minor. C. B. 160. Offic. 366.

1937 Polypodium vulgare. C. B. 359.
Polypodium quercinum. Offic. 379.

1938 Ranunculus ceratophyllus, ſeminibus falcatis in ſpicam adactis. Mor. Hiſt. 2. 440.

1939 Rhus foliis ternatis lineari-lanceolatis, integerrimis petiolatis utrinque glabris.

1940 Salvia foliis cordatis obtuſis crenatis ſubtomentoſis, corollis calyce anguſtioribus. Linn. Sp. Pl. 25.

1941 Scordium alterum, ſive Salvia ſylveſtris.
Scorodonia. Offic. 438. C. B. 247.

1942 Scrophularia major. Park. 610.
Scrophularia nodoſa fœtida. C. B. P. 235. Offic. 440.

1943 Scrophularia aquatica major. C. B. P. 235.
Betonica aquatica. Ger. 579. Offic. 441.

1944 Solanum caule inermi ſubfruticoſo, foliis oblongo ovatis ſinuatis utrinque glabris floribus alaribus. Miller's Icons.

1945 Siſarum Germanorum. C. B. 155. Off. 456.

1946 Sium arvenſe ſive ſegetum. Tourn. 308.
Selinum Si foliis. Ger. emac. 1018.

1947 Tormentilla ſylveſtris. C. B. 326. Off. 489.

1948 Valeriana floribus tetrandis æqualibus, foliis pinnatifidis, ſeminibus palea ovali adnatis. Hort. Upſal. 13.

1949 Veratrum flore ſubviridi. Tourn. 272. Offic. 226.

1950 Viburnum foliis ovatis acuminatis ſerratis venoſis, petiolis lævibus.

XV. *An Account of the* Cicuta, *recommended by Dr.* Storke; *by* William Watſon, *M. D. F. R. S.*

To the Royal Society.

Gentlemen,

Read Feb. 17, 1761.

IN a paper I lately laid before you, I endeavoured to demonſtrate, that the Cicuta major, which, ſince the publication of Dr. Storke's work at Vienna, had been uſed medicinally in England, was the plant intended by that gentleman; and not the Cicuta aquatica, as had been ſuggeſted by ſome practitioners here. And Dr. Storke has removed every doubt, which could remain, by tranſmitting hither to Mr. Hudſon, a very ingenious apothecary and botaniſt, ſome leaves of the Cicuta major, or common hemlock, which grew at Vienna, and is of the ſame ſpecies with the plant ſo denominated here.

As Dr. Storke informs us, that, ſince the publication of his treatiſe, he has received letters from almoſt every part of Europe, confirming his good opinion of the virtues of the Cicuta, and as he is about to publiſh a ſecond treatiſe upon the ſame ſubject, containing ſtill more extraordinary relations of cures brought about, by adminiſtring that plant; there is no doubt therefore, but that endeavours will be made here, to confirm the truth of the doctor's aſſertions; more eſpecially, as ſome of the diſeaſes, in which Dr. Storke found the Cicuta attended with great ſucceſs, are ſuch as are of all others the moſt ſhocking to human nature, and have, by too long experience, been found to give way to no other means.

Hence it is highly important to every one, more particularly to phyſicians, that the very plant, directed by Dr. Storke, be adminiſtred, and no other in the place of it, either through inattention or want of knowlege; as judgment in the phyſician is of no real ſervice, unleſs his preſcriptions are faithfully prepared.

For theſe reaſons, it may not be improper to inform thoſe medical practitioners, who are not converſant in botany, and who may nevertheleſs be deſirous of trying the effects of the Cicuta, that at this time of the year there is another plant, growing in the ſame places, and often mixed with it, ſo much reſembling it in appearance, as not, without ſome attention, to be diſtinguiſhed from it; which, however, greatly differs from it in ſenſible qualities. Great care therefore ought to be taken, that the one of theſe ſhould be ſelected from the other.

The plant ſo much reſembling hemlock, is the Cicutaria vulgaris of the botaniſts, which in ſome parts of England is called cow-weed, in others wild cicely. Its greateſt reſemblance to hemlock is in the ſpring, before the ſtalks of the leaves of the hemlock are interſperſed with purple ſpots; and therefore, at that ſeaſon, more eaſily miſtaken for it; though, even then, the leaves of the hemlock ſmell much ſtronger, are more minutely divided, and are of a deeper green colour, than thoſe of the cow-weed. Afterwards, indeed, they are more eaſily diſtinguiſhed, as the Cicutaria flowers at the end of April and beginning of May, and the Cicuta not till June, when the other is paſt: to ſay nothing of the flowering ſtalk of the cow-weed being furrowed, and ſomewhat downey; and that of the hemlock, ſmooth, even, and always ſpotted. Theſe plants differ likewiſe very eſſentially in their ſeeds, which in the cow-weed are long, ſmooth, and black, when ripe; whereas thoſe of the hemlock are ſmall, channelled, and ſwelling towards their middle.

Beſides the cow-weed, there is another plant in appearance very like the hemlock, although evidently differing from it in other reſpects; and, unleſs I am very greatly miſinformed, quantities of this have been collected, and ſold in London for the hemlock. This is more likely to be taken for the hemlock in ſummer or autumn, as it is an annual plant, and is produced and flowers late in the ſeaſon. The plant here meant is the Cicuta minor of Parkinſon, or Cicutaria tenuifolia of Ray. This, however, is eaſily diſtinguiſhed from hemlock, by its leaves being of the colour and ſhape of parſley, its flowering ſtalks having no purple or other ſpots, and not having the ſtrong ſmell peculiar to hemlock.

To the two plants before-mentioned, may be added a third, which very frequently, more eſpecially about London, grows along, and is mixed, with the hemlock. This plant is called, by the late excellent Mr. Ray, Small hemlock-chervil with rough ſeeds; and is denominated by Caſpar Bauhin, in his Pinax, Myrrhis ſylveſtris ſeminibus aſperis. This, like the cow-weed before-mentioned, can only be miſtaken for hemlock in the ſpring. It may be diſtinguiſhed then from it, by the leaves of the myrrhis being more finely cut, of a paler green colour, and, though they have ſomewhat of the hemlock ſmell, are far leſs ſtrong, and have no ſpots. This plant flowers in April, and the ſeeds are ripe before the hemlock begins to flower; and theſe ſeeds are cylindrical, rough, and terminate in an oblong point.

The leaves of hemlock are moſt fit for medicinal purpoſes, as being in their greateſt perfection, when collected in dry weather, from the middle of May to the time that their flowering ſtems begin to ſhoot; as by that time the plants will have felt the effects of the warm ſun, have acquired an highly viroſe ſmell, and the ſtems of the leaves are covered with purple ſpots, an argument of the exaltation of their juices: and we ſhould be attentive here to give them all theſe advantages, as three degrees of latitude, and other circumſtances of ſoil and ſituation, may occaſion a very ſenſible difference in the qualities of the ſame plant; an inſtance of which occurs in the plant under conſideration, and may be one of the cauſes, why the effects of the hemlock have not been ſuch here, as we are aſſured they are at Vienna; viz. Dr. Storke ſays, that the root of hemlock, when cut into ſlices, pours forth a milky juice, which I have never ſeen it to do here in England.

There are ſeveral vegetables, which, though they thrive apparently well, their productions are, nevertheleſs, not the ſame as in other parts of the world, where the heat is more intenſe, and the ſummers are of longer continuance. It would be extremely difficult here, though the plants thrive very well, to produce from the white poppy, or Ciſtus ladanifera, either the opium or the labdanum, the known production of theſe vegetables in other parts of the world. No art can make here the tragacantha pour forth its gum, the lentiſcus its maſtic, or the candleberry myrtle of North America its ſebaceous concrete. To theſe might be added many others, too tedious to mention.

In ſuch mild winters as the laſt, the leaves of hemlock may be procured in any part of them; but they are not to be depended upon, as their ſpecific ſmell is then comparatively weak, their juices poor and watery, and they are wholly without ſpots.

I am,

With all poſſible regard,

Gentlemen,

Your moſt obedient

humble ſervant,

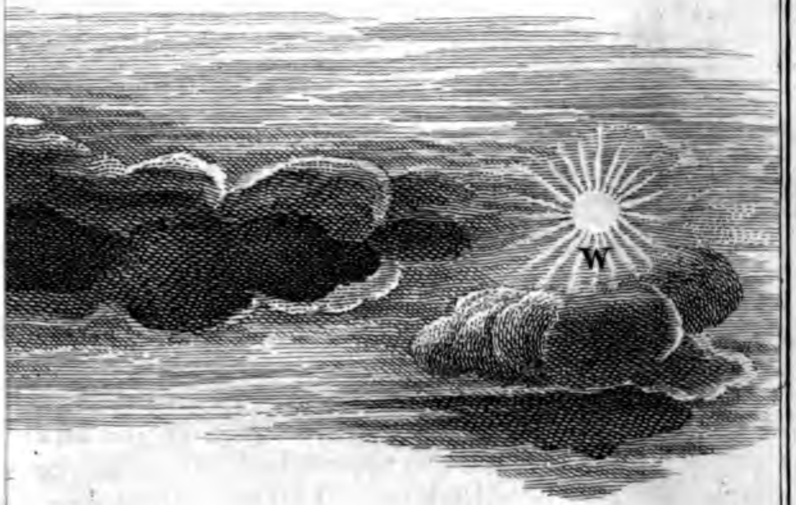
W. Watſon.

XVI. *An Account of an* Anthelion *obſerved near* Oxford. *In a Letter to the Reverend* Tho. Birch, *D. D. Secretary to the Royal Society, from the Reverend* John Swinton, *B. D. of* Chriſt-Church, Oxon. *F. R. S.*

Good Sir,

Read Feb. 19, 1761.

RETurning home with the Revd. Mr. Jane, Student of Chriſt-Church, from Cudſden, where we had been to make a viſit to the Biſhop of Oxford, on Thurſday, July 24th, 1760; we reached the top of Shotover-hill, about 10′ paſt 7 o'clock in the evening. At 7h 12′ I accidentally diſcovered a luminous appearance, not much unlike the ſun when ſeen through clouds, about four or five times as big as the ſolar diſk. [*Vid. Tab.* II.] The ſun was then pretty reſplendent, though a full exertion of its rays was ſomewhat obſtructed by a thin wateriſh cloud. Soon after a very diſtinguiſhable *Mock-Sun*, oppoſite to the true one, which I take to have been an *Anthelion*, appeared. This was not however completely formed, that part of its diſk remoteſt from the ſun being indiſtinct and but ill defined. Nor could the figure of the lucid tract round it, though approaching a circle, be with any preciſion aſcertained. This uncommon meteor was ſeated in the E. but the ſun had a weſterly ſituation. From 7h 12′ to 7h 18′ the phænomenon ſhone very conſpicuouſly, though almoſt ſurrounded by dark thickiſh clouds. The diſk of the *Spurious Sun* ſeemed as large and bright as that of the true one, but was not



J. Mynde sc.

not ſo well defined. Between $7^h\ 18'$ and $7^h\ 28'$ the meteor was more than once partially obſcured, by the circumjacent clouds; a very thick black one, which had been viſible from the moment I firſt perceived the phænomenon, then extending itſelf about from the weſtern limb or edge of it to the ſun. From the beginning to the end of the *Mock-Sun*'s appearance to us, about 18′, there was much clear ſky above the ſun, even up to the zenith, and thick duſky clouds below it; but the tract both above and beneath the meteor was, for the moſt part, covered with ſuch clouds. This might perhaps be the reaſon why only ſome very faint traces of one of the two coloured arches, by whoſe interſection the *Anthelion* was formed, which generally attend this kind of phænomena, were to be diſcerned. When in its moſt refulgent ſtate, the *Anthelion* was as yellow as the ſun; but the lucid tract ſurrounding it was of a paler yellow, or whitiſh caſt, interſperſed with a few reddiſh and ſubfuſcous ſpots. The whole, when leaſt affected by the neighbouring clouds, ſeemed in extent to be quadruple, if not quintuple, the ſpace occupied by the diſk of the ſun. In fine, the phænomenon was ſometimes brighter, and ſometimes more obſcure; varying, through the whole courſe of its duration, according to the variation of the atmoſphere and the clouds. At laſt, after ſeveral ſhort ſucceſſive intervals of brightneſs and partial obſcurity, it was abſorbed by the black cloud above-mentioned, nearly connecting it with the ſun; and, juſt as we came to the bottom of the hill, about $7^h\ 30'$, totally diſappeared.

The wind, during the whole continuance of the *Anthelion*, was almoſt full N. as it had been the greateſt part of the day. The weather was for this time of the year remarkably cold, and much colder than it had been for above a month before. There was even that morning a ſmart white froſt, and in ſome places ſmall collections of particles of ſnow, though four or five of the preceding days were exceſſively hot. The wind was not high on the 24th, but ſomewhat ſharp. It was a bright ſun-ſhiny day, reſembling a clear froſty day in December; but not, by ſeveral degrees, ſo cold. The following night the air ſeemed ſtill replete with the ſame ſort of particles that had chilled it the day before. Hence will farther appear the probability of the moſt received opinion, relative to the formation of this kind of meteors; which makes them to proceed from a multitude of minute icy or ſnowy particles ſuſpended in the air, and either refracting or reflecting the ſolar rays in ſuch manner as to multiply the image of the ſun. However the theory of *Anthelia*, for want of a proper number of obſervations, ſeems not yet to be arrived at ſuch a degree of perfection as by every lover of phyſiology could be deſired.

Inſtances of *Anthelia* are extremely rare. I have hitherto been able to meet with only two of them, viz. that obſerved near Dantzick (1) by Hevelius, Sept. 6th, N. S. 1661. and that ſeen at Wittemberg in Saxony, Jan. 18th, N. S. 1738. a deſcription of which was ſoon after communicated to the Royal

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(1) Johan. Hevel. *Phænomen. Aer.* p. 174, 176. Gedani, 1662.

Society (2) by J. Frid. Weidler, Profeſſor of Mathematics there. The former of theſe meteors appeared from 6h to 6h 15′ in the evening, the ſun being then poſited in the W. and the *Anthelion* in the E. the other from 9h 30′ to 9h 45′ in the morning, the ſun being at that time S. and the phænomenon N. *Anthelia* therefore being ſo ſeldom obſerved, and yet obſervations of them being ſo neceſſary, in order to aſcertain the theory of this ſpecies of meteors; I was inclined to believe, that the account now tranſmitted you, rude and imperfect as it is, might yet not be altogether unacceptable to the Royal Society. I can only anſwer for the fidelity of the relation, and wiſh a more perfect one had been drawn up by a perſon better qualified to obſerve the phænomenon here deſcribed, that it might have been more worthy the attention of the learned and illuſtrious body, to whom I have the honour of communicating this paper. If the meteor could have been viewed from the firſt to the laſt moment of its exiſtence, perhaps other circumſtances, proper to be known, for the happier inveſtigation of its cauſe, might have occurred. But this amounting to little more than a bare poſſibility, I ſhall content myſelf with having juſt hinted it here; and only beg leave to add, that

I am, with the higheſt regard and eſteem,
SIR,
Your moſt obedient
humble ſervant,
John Swinton.

Chriſt-Church, Oxon.
July 28, 1760.

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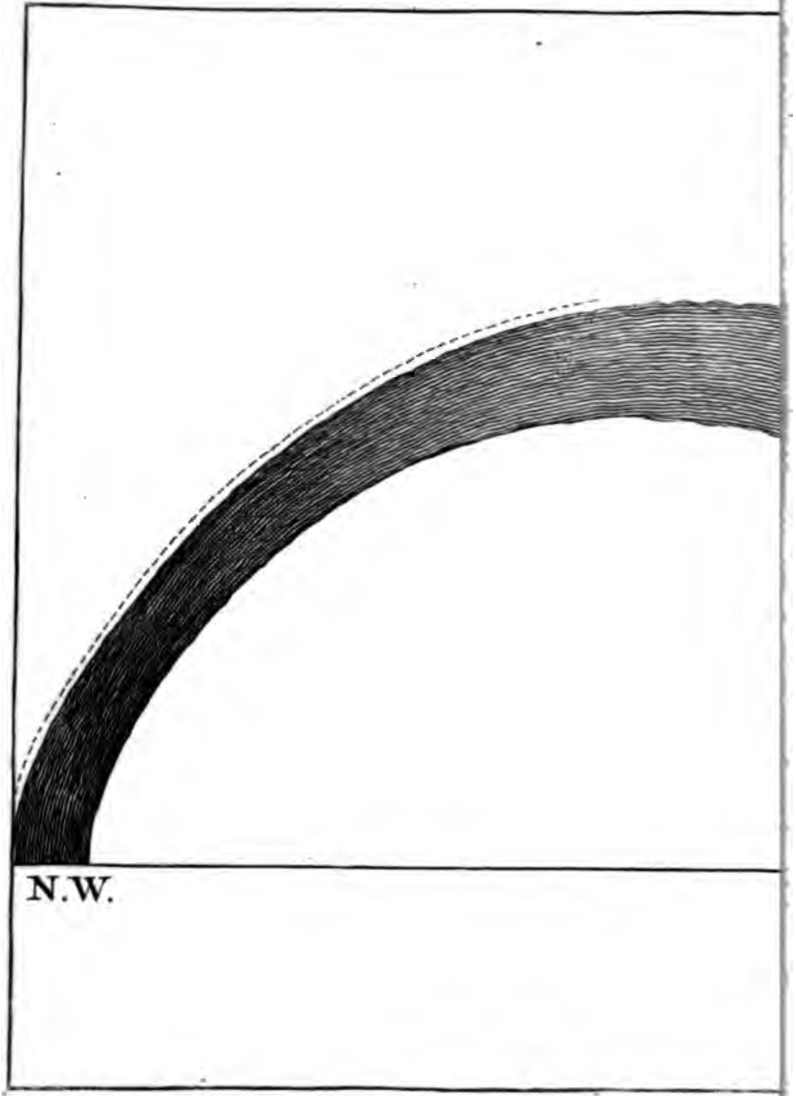
(2) *Philoſ. Tranſact.* N° 454. p. 221. *July*, &c. 1739.

XVII. *An Account of a Production of Nature at* Dunbar *in* Scotland, *like that of the* Giants-Cauſeway *in* Ireland; *by the Right Reverend* Richard *Lord Biſhop of* Offory, *F. R. S.*

Read Feb. 26, 1761.

THE paſſage into the harbour of Dunbar is very narrow, between two rocks: one of them is the eaſt ſide of the harbour; the other is a promontry, ſtretching out about a hundred yards to the north, and is about twenty yards wide, having the ſea on each ſide of it, when the tide is in. This head is a moſt extraordinary natural curioſity: it is of a red ſtone, which is not a lime-ſtone, but appears rather like a very hard free-ſtone. It looks on both ſides like the Giant's-cauſeway in Ireland: the ſtones on the weſt ſide are from a foot to two feet over; on the eaſt ſide they are larger, from two feet to four feet. I obſerved the pillars from three to eight ſides; but only one or two of the firſt and laſt: they may be ſaid to be in joints, but are ſtrongly cemented together by a red and white ſparry ſubſtance, which is formed in laminæ round the pillars, and between the joints, two or three inches in thickneſs. The interſtices between the large pillars, which are but few, are filled with ſmall pillars, without joints. The pillars conſiſt of horizontal laminæ: the joints are not concave and convex when ſeparated, but uneven and irregular: they lie ſloping from eaſt to weſt: on the weſt ſide, towards the end, the pillars become very large and confuſed,



N.W.

as I ſaw them to the eaſt of the Giants-cauſeway, and in the iſle of Mull; except that theſe are divided by ſuch a ſparry ſubſtance into a great number of ſmall figures, which ſeem to go down through them. There are ſpots and veins of a whitiſh ſtone in the pillars. There is no ſign of any thing of this kind in any of the rocks near, that I could obſerve, or hear of.

XVIII. *An Account of a remarkable Meteor ſeen at* Oxford. *In a Letter to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society, from the Rev.* John Swinton, *B. D. of* Chriſt-Church, Oxon. *F. R. S.*

Reverend Sir,

Read Feb. 26, 1761.

BEING on the Parks, or public univerſity-walk here, on Sunday, Sept. 21, 1760, from $6^h\ 40'$ to $7^h\ 25'$ P. M. ſuch a meteor exhibited itſelf to my view as I had never ſeen before. [*Vide Tab.* III.] A dark cloud, like a pillar or column of thick black ſmoke, and perpendicular to the horizon, appeared in the N. W. puſhing gradually forward towards the zenith, and at laſt extending itſelf almoſt to the oppoſite part of the heavens. It was at firſt ſeveral degrees broad, but grew broader and broader, as it approached the zenith; through which it paſſed, and nearly biſſected the hemiſphere, in a wonderful manner. At 7^h this ſurprizing arch, falling little ſhort of a ſemicircle,

that

that would have reſembled an *Iris*, had not the colours of it been different, ſeemed to be completely formed. I ſay, "had not the colours of "it been different;" becauſe the lower part was exceeding black, but the other ſubfuſcous only and white. The exterior limb of this arch as far as the vertex was tinged with a pale yellow, that gave it no diſagreeable appearance. The edges of it were at firſt tolerably ſmooth, and pretty well defined, but afterwards became rugged and irregular. The whole moved with the wind, from the firſt to the laſt moment of its exiſtence. For a few minutes, it rendered the moon abſolutely inviſible. That planet had, for a conſiderable time before its approach, been ſomewhat darkened by the thick hazy air; which, however, did not totally obſcure it. The tract near the northern part of the horizon, contiguous to the meteor, was interſperſed with fuſcous caliginous clouds, and that near the zenith with ſome of a whitiſh colour. All of them were very diſtinguiſhable from the phænomenon itſelf. They grew gradually paler and paler, till they were intirely diſperſed. About 7h 25′ P.M. all remains of the meteor were ſo perfectly diſſipated, that not the fainteſt traces of them were to be ſeen.

That this phænomenon was a *Water-Spout*, or rather the firſt appearance of one, though the proper *Spout* itſelf was not viſible, will perhaps not be denied by any perſon moderately verſed in natural hiſtory. The foregoing deſcription ſeems to render this at leaſt extremely probable. This meteor made a conſiderable impreſſion upon the minds of the vulgar here. Several of the lower ſort of people, according to cuſtom, believed it to portend ſome calamitous event.

One of them declared, that it would prove a ſcourge (as ſhe imagined it to reſemble a whip or ſcourge) to this nation; and others, even leſs ſuperſtitious, were ſtruck with no ſmall degree of aſtoniſhment at ſo unuſual a ſight. The weather was mild, or rather warm, the whole day. The wind, during the continuance of the phænomenon, and almoſt ever ſince, was W. S. W. though it did not then exceed a very gentle gale.

When or where the diſſolution of the *Spout* happened, provided we admit of the foregoing ſuppoſition, I cannot pretend to ſay; not having received, from any perſon, the leaſt information on that head. The weather for three months before was, with very little intermiſſion, hot and exceeding dry, ſuch as generally precedes meteors of this kind *. As the phænomenon was ſeen, by the Reverend Dr. Neve, Fellow of Corpus-Chriſti College, at Middleton-Stoney, twelve miles from hence, and (as I was told by Samuel Wilmot, Eſq;) at Sandford, N. W. of that village, a few miles farther from this place, at the very time that I obſerved it, and attended by circumſtances nearly the ſame with thoſe that occured to me; it muſt have been, as might eaſily be demonſtrated, of a pretty conſiderable height.

Perhaps it would be difficult to find an account of a meteor reſembling this in every particular, either in antient or modern hiſtory. 'Tis certain, a ſimilar one is not remembered, or recorded, ever to have been ſeen here. Such appearances at ſea, on our coaſts, are very uncommon; but at land, eſpecially ſo far diſtant from the ſea as is Oxford, extremely rare. I therefore judged, that a ſhort deſcription of it might not be altogether unacceptable to the Royal Society. I ſhall only beg leave to add, that a moſt terrible ſtorm of rain and hail followed it, which continued from a little paſt 3 to near 5 o'clock, the next morning; that we have had much of ſuch ſtormy weather here, and in the neighbourhood of this city, ever ſince; and that

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* *Philoſ. Tranſact.* Vol. XXII. n. 270. p. 805.

I am,

With all poſſible conſideration and eſteem,

Good Sir,

Your moſt obliged,

and moſt obedient,

humble ſervant,

John Swinton.

Chriſt-Church, Oxon,
Sept. 27, 1760.

XIX. *An Account of ſome Productions of Nature in* Scotland *reſembling the* Giants-Cauſeway *in* Ireland: *In a Letter to the Right Reverend* Richard *Lord Biſhop of* Oſſory, *F. R. S. from* Emanuel Mendez da Coſta, *F. R. S.*

To the Right Reverend Richard (Pococke) *Lord Biſhop of* Oſſory.

Mincing-Lane in Fenchurch-Street,
March 5, 1761.

My Lord,

Read March 5, 1761.

YOUR Lordſhip having communicated to the Royal Society, at their laſt meeting, an account of ſome rocks at the entrance of the harbour of Dunbar in Scotland, which are formed into pillars, like the growth of the famous Giants-cauſeway, but which are ſolid, and not joined like them, I take the liberty to ſend your Lordſhip the following account of a like natural production in other parts of Scotland, which was communicated to me by my ingenious friend Mr. Murdoch Mackenzie, who, by order of the Lords of the Admiralty, ſurveyed the coaſts of that kingdom, and which came too late to be inſerted in its proper place in my work.

In Cana iſland, which is four Engliſh miles long, to the ſouthward of Skye, and near the iſland of Rum, the rocks, about a quarter of a mile above the harbour, riſe into polygon pillars ſouthward. About two miles from the weſt end of Cana, is a low rock, or ſmall iſland, where is a very regular pavement of hexagon ſtones, each about a foot deep, and about nine inches over. They form a ſmooth uniform pavement; and the ſides of all the ſtones lie extremely contiguous, or cloſe. Immediately below this upper pavement, lies another exactly like it. The pillars are jointed exactly like thoſe of the Giants-cauſeway, and are laid with their concavities downward, and their convexities upward; and their hollows are as much in proportion to theſe pillars, which are ſmaller, as they are in thoſe of the Giants-cauſeway. Theſe places are about 200 miles northward diſtant from the Giants-cauſeway.

If your Lordſhip chuſes to communicate this account to the Royal Society, it is at your Lordſhip's pleaſure.

I am,
With great reſpect,
My Lord,
Your Lordſhip's
Moſt devoted and
obliged humble ſervant,

Emanuel Mendez da Coſta.

XX. *Elements of new Tables of the Motions of* Jupiter's *Satellites: In a Letter to the Reverend* Charles Maſon, *D. D.* Woodwardian *Profeſſor in the Univerſity of* Cambridge, *and F. R. S. from Mr.* Richard Dunthorne.

SIR, Cambridge, March 3, 1761.

Read March 5, 1761.

THE public employment *, wherein I am at preſent, and for ſeveral years paſt have been, engaged, not permitting me to make new tables of the motions of Jupiter's ſatellites, according to the laſt corrections I had (from a compariſon of more than eight hundred obſervations) made in the places and orbits of thoſe planets, I am at laſt perſuaded to communicate, by your means, to the Royal Society, the elements of thoſe tables, hoping they will prove no unacceptable preſent to aſtronomers.

The tables are deſigned upon the plan of thoſe of Mr. Pound for the firſt ſatellite, publiſhed in the Philoſophical Tranſactions, N° 361. except that I have not deducted the greateſt equations from the epochs, as is done by Mr. Pound.

The epochs of the conjunctions of the ſeveral ſatellites with Jupiter, fitted to the Julian year (before the alteration of the ſtyle in England), and to the meridian of the Royal Obſervatory at Greenwich, are as follows.

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* That of ſurveyor to the corporation of the great level of the fens.

Jul. years cur-rent.	Conj. 1ſt ſat.				Num. A.	Num. B.	Num. C.	Conj. 2d ſat.				Conj. 3d ſat.				Conj. 4th ſat.			
	D.H.		′	″				D.H.		′	″	D.H.		′	″	D.H.		′	″
1728	0	21	58	16	630	651	484	0	21	20	0	6	4	57	0	2	3	25	0
1748	0	3	7	18	316	962	175	3	2	32	24	3	18	7	54	1	16	37	41
1768	1	2	44	57	2	278	869	1	18	26	54	1	7	18	48	1	5	50	22

Number C is the period of 437 days (wherein the three innermoſt ſatellites return very nearly to the ſame ſituation in reſpect of one another, and of Jupiter's ſhadow), in milleſimals of a circle; and muſt be corrected by the equation of number B, under a contrary title.

The ſecond ſatellite has a ſynodical equation of 16′ or 17′ in time (whoſe revolution is in this period), to be ſubtracted, if numb. C be leſs than 500; added, if greater. The firſt and third ſatellites have alſo ſmall ſynodical equations (returning in the ſame period), that of the firſt ſatellite being about 3′, of the third about 2′ in time; both to be added, if numb. C be leſs than 500; ſubtracted, if greater.

The orbit of the third ſatellite is manifeſtly excentric, as well as that of the fourth. Its apojovium in 1728 was about 10° of ♈, and moves forward 35° in 20 years: its greateſt equation is about 15′ in the ſatellite's orbit, or 7′ in time.

The apojovium of the fourth ſatellite in 1728, was in 12° 30′ of ♓, and moves forward about 12° in 20 years: its greateſt equation is 53′ in the ſatellite's orbit, or 59′ in time.

I found no reaſon to make any alteration in the ſemi-durations of the eclipſes of the firſt ſatellite from Mr. Pound's tables.

The greateſt ſemi-durations of the eclipſes of the ſecond, third, and fourth ſatellites in the nodes, are $1^{h}\ 27'$, $1^{h}\ 47'$, and $2^{h}\ 24'$ *, reſpectively.

The nodes of the ſecond ſatellite ſeem to be at reſt in about 50° of ♒ and ♌; but the inclination of its orbit varies from 2° 50′ to 3° 52′: it was leaſt in 1668, greateſt in 1715, and ſeems to have been at its greateſt and leaſt once in the intermediate years. I ſuppoſe it at the leaſt in 1730.

The nodes of the third ſatellite in 1727, were in $16\frac{1}{2}^{\circ}$ of ♒ and ♌, and move forward about $2\frac{1}{2}^{\circ}$ in 20 years: the inclination of its orbit in 1695 was 3°, and has been increaſing ever ſince: it ſeems as if it would get to its maximum about 1765, and would then be about 3° 24′.

The nodes of the fourth ſatellite in 1730 were in $13\frac{1}{2}^{\circ}$ of ♒ and ♌, and move forward 2° in 12 years: the inclination of its orbit is about 2° 40′, and does not ſeem to vary above one or two minutes either way.

From theſe elements, it will be eaſy for any perſon, moderately ſkilled in ſuch matters, to conſtruct tables of the motions of the ſatellites in the method of Mr. Pound, which may be ſeen in the latter part of Halley's tables.

I am, SIR,

Your humble ſervant,

Richard Dunthorne.

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* The ſemi-durations of the eclipſes of the fourth ſatellite will be about 2′ more at the aſcending, and 2′ leſs at the deſcending node, on account of the excentricity of its orbit.

XXI. *Diſſertationem hanc de* Zoophytis, *Regiæ Societati Scientiarum* Angliæ *legendam et judicandam præbet* Job Baſter, *Med. Doctor. Acad. Cæſ. Reg. Soc.* Angl. *et* Holland. *Socius.*

Read April 2, 1761.

QUUM varia multumque inter ſe diſcordantia, quæ de coralliorum, lithophytorum, corallinarum, et id genus naturæ operum origine et propagatione, aliquot abhinc annis prodierunt, ſcripta perlegens, pleroſque eorundem authores hæc animalculorum, quibus adhærere deprehenduntur, opus et fabricam exiſtimare, animadverterem, aliis tamen diſſentientibus, veraſque eſſe plantas contendentibus; ipſe quidem dubius corpora hæc examinare, propriiſque perſcrutari experimentis, decrevi.

Quo facto, tot perfectæ vegetationis ſigna, tamque ſolida reperi argumenta, ut eadem animalculis, adeo minutis, ut, niſi boni ope microſcopii conſpici, nequeant, tamque ſimplicibus, ut pauciſſimis tantum gaudeant membris, ad opus ullum perficiendum plane ineptis, ſuam debere originem, exiſtimare nequaquam potuerim.

Simul tamen, hæc animalcula, apicibus corallinarum non incidere tantum, ſed ita cohærere quodammodo, experiebar, ut adhucdum dubius, nihil ſatis certi definire aut pronunciare auderem; donec vir magnus Linnæus, novo has tenebras lumine diſpellens, ſubſtantias has zoophytorum nomine indigitabat:

bat: id eſt, "Compoſita animalcula, in bivio ani-
" malium vegetabiliumque conſtituta, radicata plera-
" que cauleſcunt, multiplicata vita ramis, gemmis
" cæduis, metamorphoſique florum animantium,
" ſponte ſeſe moventium, in capſulas ſeminiferas
" tranſeuntium. Ac ſi plantæ eſſent zoophyta, ſenſu
" motuque deſtituta; et zoophyta veræ plantæ, ſed
" ſyſtemate nerveo, ſenſus motuſque organo, in-
" ſtructæ (*a*)."

Ulteriori examine hæc mihi digna videbatur ſententia, quia mire res alias illuſtrabat, quæ antea obſcuræ et incomprehenſibiles mihi fuerant viſæ. Novo itaque ſtudio et attentione hæc naturæ opera iterum cæpi perſcrutari; ſi forte de eorum origine et propagatione alterius quid certi comperire poſſem.

Haud ignorabam, qua ratione natura ab hominibus ad animalia procedat, quæ intellectu atque ſenſibus parum a nobis differunt: quaque eadem parvis ad talia animalia deſcendat gradibus, quæ vix ullam vitam aut motum habere videntur.

Naturæ etiam ſcrutatoribus difficillimum eſſe moleſtiſſimumque noveram, regnum animale in determinatas claſſes, genera et ſpecies diſtinguere: dum inter ea, quæ determinatis caracteribus diſcreta, et certo quaſi orbi incluſa ſunt, ſemper intermediæ quædam ſpecies reperiuntur, quæ utriuſque proxime accedentis ſpecieï quid poſſideant, et ita copulationem quaſi duarum diverſarum ſpecierum conſtituant: colorum ad inſtar, qui ita commiſcentur et quaſi pereunt, ut nemo veros cujuſque fines determinare poſſit.

(*a*) Syſtema Naturæ. Edit. dec. p. 643.

Subibat etiam acutiſſimum philoſophum Leibnitſium ex lege continuitatis prævidiſſe jam et prædixiſſe, hiſtoriæ naturalis perveſtigatione, olim corpora repertum iri, quæ æquo jure animalibus ac plantis acenſeri poſſunt: quia corpora quæcunque creata non niſi unicam ſiſtunt catenam, quorum quippe diverſæ ſpecies, inſtar annulorum diverſorum, tam arcte ſociatæ et copulatæ ſunt, ut ne ſagaciſſimi quidam ſenſus, nec ipſa denique imaginatio, ubi unum incipiat, aut alterum deſinat, definire poſſint.

Planta eſt corpus organicum, expers ſenſus et motus ſpontanei (*b*), conſtans tamen vaſis et humoribus, ope radicis (*c*) corpori cuidam adhærens, unde vitæ et incrementi materiam nanciſcitur.

Animal eſt corpus organicum, quod ſenſu et perceptione præditum, ſponte ſua motus quoſdam, ſibi proprios, edere poteſt.

Hæ definitiones, quantumvis inter ſe diverſæ, in zoophytis tamen conveniunt: radice corpori cuidam adnexa creſcunt, et tamen ſimul ſunt animalia, quæ tacta ſe ſentire oſtendunt, et eſcam ſibi convenientem

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(*b*) Motum, qui in tangendis herbæ vivæ ſive mimoſæ foliis, maturis balſaminæ cellulis ſeminalibus, coverſione heliotropii, anemonoidis, &c. ad ſolem, in floribus et foliis quibuſdam ſub veſperam ſe contrahentibus obſervatur, his objici poſſe non arbitror, quoniam is mere mechanicus, non ſpontaneus eſt.

(*c*) Dantur tamen plantæ, quarum radices nulli adhærent corpori, quæque iiſdem plane carere videntur: prioris generis ſunt hyacinthi, &c. quoties bulbis, inferiore parte ſurſum converſa, vitro aqua repleto, inſeruntur, non minus læte creſcentes et florentes ac alii, quorum flos ſurſum enaſcitur: ad poſterius genus pertinent plantæ, quæ ſemper aquæ innatare videntur.

conſpicantes, quorumdam membrorum motu, arripiunt et devorant.

Ambas has qualitates, tam diverſas, manifeſto in zoophytis expertus, quam primum priſtinam meam de his corporibus ſententiam, prout, experientia duce, veritati magis congruere videbam, mutare non dubitavi.

Duo zoophytorum, genera Linnæus ſtatuit, dura ſive lapidea, ut tubipora, millepora, madrepora, quæ inter et corallium rubrum locum obtinet; et mollia, ut iſis, gorgonia, alcyonium, tubularia, eſchara, corallina, ſertularia, pennatula, hydra, tænia, volvox (*d*) quorum octo priora, cum in mari inveniantur, in eorumdem ego naturam et originem, quatenus hic, in Zeelandia, acquiri poſſint, aliquando inquirere, in animum induxi.

Primo proprie ſic dictas corallinas examinare diſerſus, omnes earum, quotquot mihi oblatæ ſunt ſpecies, non zoophyta, quamvis Linnæus iiſdem adnumeret, ſed veras è confervarum genere plantas eſſe, luculentiſſime perſpexi.

Numquam in earum apicibus polypi inveniuntur: ſemen contra cellulis incluſum (*e*), eodem, quo aliæ plantæ marinæ, modo produnt. Quod ipſum clariſſime vidi, in

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(*d*) Quod ſi definitum definitioni omnino reſpondere debet, ex laudata modo Linnæi definitione pennatula, hydra, tænia, volvox, vocari zoophyta nequeunt (vera ſunt animalia), quia plane nihil in ſe habent radici ſimile, multo minus eadem ulli adhærent corpori.

(*e*) Vide Opuſcula mea Subſeciva, Tab. i. fig. 3.

Corallinâ dichotomâ, capillari, articulis cylindricis, breviſſimis, dichotomiæ, ſubclavatis. Linn. N° 7. Sive *Corallinâ ramulis dichotomis, teneris capillaribus rubentibus.* Elliſ. Tab. xxiv. N° 5. e. E.

Corallinâ dichotomâ, capillaris articulis omnibus clavatis. Linn. N° 8. Sive *Corallinâ dichotomâ, capillis denſis, criſtatis ſpermophora.* Elliſ. Tab. xxiv. N° 6. f. F. In

Corallina capillari, inferne pinnata, articulis cylindricis. Linn. N° 9. Sive *Corallinâ alba ſpermophora, capillis tenuiſſimis.* Elliſ. Tab. xxiv. N° 7. g. G.

Atque de omnibus à Linnæo enarratis corallinarum ſpeciebus, ſolidiſſimis adductus rationibus ſuſpicor, licet omnes illas explorandi occaſio nondum mihi contigerit.

Penicillus, ſive *Corallina culmo ſimplici, ramis faſciculatis, faſtigiatis, dichotomis flexilibus inarticulatis.* Linn. N° 10. Sive *Corallina tubularia Melitenſis.* Elliſ. p. 92. Tab. xxxiv.

a corallinis, ſertulariis aut aliis zoophytis, plane eſt alienus: vermis enim eſt tubiphorus, ex ſcolopendrarum genere, ut clariſſime ex earumdem deſcriptione patet (*f*).

(*f*) Vide Opuſcula mea Subſeciva, lib. ii. Tab. iii. fig. 1.

Verum aliter comparata eſt ſertularia. Quod ſi attentione ſufficiente expendas, quæ de coralliorum origine doctiſſimus Donati (*g*), et ipſe de maximi generis polyporum in tubularia (*h*) olim dixi; a primo uſque initio perfectam omnino adeſſe vegetationem, perſuaſſimum tibi feceris. Liquet ſane, ovula hæcce tenerorum inſtar geniculorum, novorum inſtar membrorum, matris ex corpore pullulare, poſtea majora fieri, ramuſculorum more ſuccreſcere, et tandem ad maturitatem perducta decidere, et lapidem, concham aut corpus aliud durum offendentia, vel glutinoſa, qua inveſtita ſunt, cruſta, vel inæquali ipſius corporis ſuperficie, donec fœtus excludantur, inhæreſcere.

Sola hujus ovuli teſta, proprie, naturâ, ſi ita loquî liceat, *vegetabilis* eſt, perinde atque alia ſemina parvas aliquot radiculas ad latus ejicit; quibus affixâ manet, et primum brevibus creſcit articulis. Internum vero hujus ovi *animale* eſt, ſimul cum teſta vegetabili, eadem ratione, eodemque tempore adoleſcit; in ramuſculos diſpergitur, è quibus temporis progreſſu, alii flores ſeu polypi prodeunt, qui ſuum rurſus ſemen ſeu ovula, prout vocare libuerit, ſuo gignunt tempore.

Id argumento omni exceptione, ut mihi quidem videtur, majori, probatum comperi, in zoophyto quodam, quod

> *Sertularia abietina*, ſeu *Sertularia denticulis ſuboppoſitis tubulatis, calycibus ovalibus, ramulis*

(*g*) Hiſtoire de la Mer Adriatique.

(*h*) Opuſcula Subſeciva, lib. i. p. 30. Tab. iii. fig. 4.

alternis. Linn. N° 5. Et *Corallinâ abietis forma*, Elliſio dicitur, Tab. i. b. B.

Et memoratam vegetationem, per quatuor fere, quibus ſervavi, menſes, clariſſime perſpexi.

Sertulariam hanc nactus, vivo coalitam oſtreo, perlucido imponebam vitro, aqua marina repleto, quæ bis, ſemel certe, quotidie commutabatur.

Quatuor circiter ſeptimanis interjectis, apicem ſertulariæ hujus haud parum extenſum, et ex nova hac parte etiam polypos enaſci, videbam.

FIG. I.

Quo minus per hoc tempus, ſertularia nigricante conſervarum cruſtâ, materiâ lanuginoſâ, ſordibuſque variis operiretur, et polypi bullacei tanta copia, in iiſdem augerentur, ut reliqui inde ſuffocarentur polypi, impedire non potui: in recentibus tamen, variis in locis prodeuntibus, et adhuc puris ramuſculis, vegetatio luculenter conſpiciebatur.

FIG. III. C.

Prius minutus emergebat articulus, qui inſtar tubuli, ad quatuor, quinque, imo octo linearum longitudinem ſuccreſcebat; elapſis aliquot diebus ad latera hujus ramuli, minores quaſi gemmulæ alternatim regulariter conſpiciebantur, quæ quatuor aut ſex dierum ſpatio, in perfectos adoleſcebant polypos.

Novo ramuſculo laterali emerſuro, polypos medii hujus ramuſculi cellulis ſuis latuiſſe, incluſique manſiſſe, mihi viſi ſunt (*i*). Cum vero teneri hujus ramuſculi

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(*i*) Röſelius, qui in dulci aqua, majori, quam ego in marina commoditate, obſervare inſecta potuit, novem in illa zoophytorum muſculi lateralis polypi adulti prodirent, omnes aliquando per totos dies, expanſis bracchiolis, ſuas extra cellulas morari videbam, præcipue affuſa, modo recenti, aqua marina.

Unde apparet, qua ratione zoophyti ſtirps, aliarum inſtar plantarum, craſſitie et proceritate augeri poſſit. Perinde ac eodem, quo illæ, modo, in longitudinem excreſcit et craſſeſcit. Sola medulla intermedia animalis eſt.

Nonne obryzum et argentum purum, arborum inſtar et cum ramuſculis per ſubſtantiam lapidoſam fodinarum, excreſcentia in iiſdem, contigit? Quanto potius animal in planta, ramorum more, creſcere poteſt? Hinc ſimul patet zoophyton omne proprio ex ſemine ſive ovulo ortum, quamdiu vivit, ſemper creſcere poſſe.

Omnibus hiſtoriæ naturalis ſtudioſis, qui etiam hæc inveſtigandi cupiditate ducuntur, ita experturos, affirmare auſim: mihi ſane poſtea ſemper ita evenit: dum modo curent, ut hæc zoophyta loco ſubfrigido et in conchis manentia ſerventur, et recenti aquâ quotidie perfundantur.

Qui vero tam procul à mari degunt, ut aqua marina difficilius potiri poſſint, cupiditati tamen ſuæ, polypis plumaceis *(polypes à panache)* in aqua dulci morantibus, obſervandis ſatisfacere licet: quod ego

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torum genera, *aſter polyporum* nomine, deprehendit. Hi polypi, quamvis ſertulariis multo minores, eodem tamen, quo hæ, creſcentes modo, omnibus etiam, quas ipſe in iis deprehendi, qualitatibus gaudebant: modo creſcendi per articulos, manſione in cellulis ante productionem novi rami, ne quidem exceptis.

eodem, quo hac deſcriptâ ſertulariâ, tempore examen inſtitui (*k*).

Ambo enim hæc zoophyta, quoad animale, externa facie ſimillima, primo obtuitu videntur eadem, ſed quoad vegetabile ſpectat, multum inter ſe differunt, quod in mari multo majus, et ramis magis, quam in aqua dulci, expanſis creſcit.

Miror igitur maxime, viros laudatiſſimos Ellisium, Juſſieum, Donatum, &c. ſertulariam tam pertinaciter opus ſive fabricam horum animalculorum vocare: cum ipſe Trembleyus (*l*), qui primus et accuratiſſime hos polypos plumaceos deſcripſit, jam dixerit, *cellulas polyporum opus non eſſe, ut tinearum cellulæ eorumdem opus ſunt, ſed, in quibus hi polypi latent, cellulas, partem corporis eorum, ſimul cum illis adoleſcentem, habendas.*

Quod ſi jam hoc Trembleyi dictum, et quæ modo de ſertularia dixi, vera agnoſcas, mox tibi perſuadebis, ſertularias animalculorum, quæ paucis ſimpliciſſimiſque membris, operari nihil, nihil efficere valent, ſed mere paſſiva, inſtar floris, inſtar coryophilli, in tubo ſuo creſcunt et proferuntur, opus neutiquam eſſe poſſe.

Neque dubitabis, quin ſertularia non tot, quot ei inſunt polypi, capitum animal (*m*); quia ſinguli

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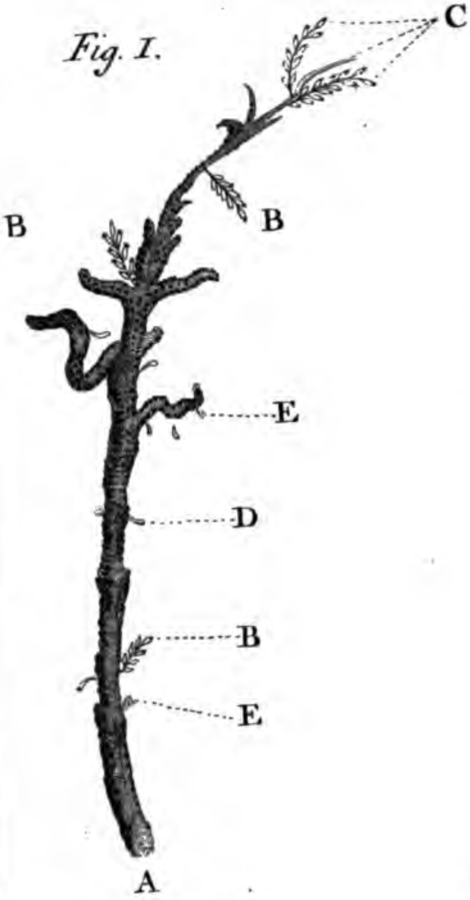
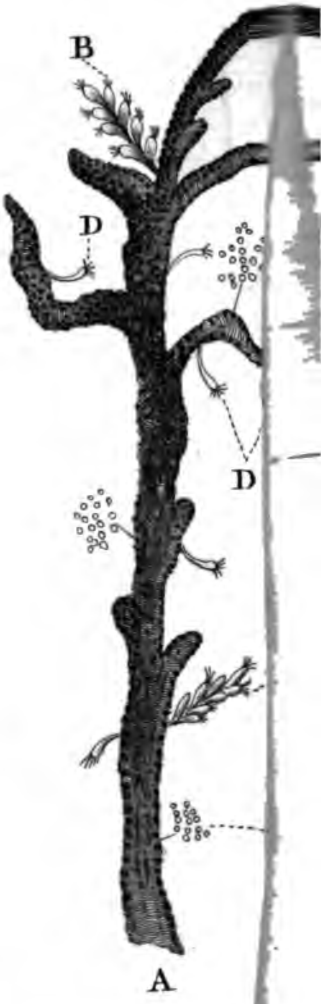
(*k*) Vel qui fide dignis aliorum teſtimoniis acquieſcit, legat laudatum Röſelii opus, cui titulus: *Inſecten Beluſtigung*, in ſupplemento ſive tomo tertio, p. 595—617. Novem zoophyta ſive ſertulariæ in aqua dulci reperiundæ, deſcribuntur.

(*l*) Memoires ſur l'Hiſtoire des Polypes.

(*m*) Hæc Donati eſt ſententia, in epiſtola ad Trembleium. Phil. Tranſ. 1757. p. 57.

Fig. 1.





polypi ſingula ſint animalia, quæ ſola natare (*n*), ſibi ſolis eſcam quærere, poſſunt; ſed potius totidem florum, qui ſemine ſuo projecto maturi decidunt, planta ſit habenda.

Liquet etiam tantum abeſſe, ut ſertulariæ polyporum procreatio eſſet, ut hi tanquam flores ſpectati, ſertulariarum potius ſint productum.

Nec minus patet, cruſtam ſive corticem, quo ſertulariæ toties tectæ reperiuntur, quique ex continuis alius, ac ſertularia gignit, polyporum generis, cellulis conſtat, proprie ad ſertularias non pertinere; ſed alienum extrinſicus allatum corpus eſſe.

Operæ pretium ergo erit, accurate inquirere, an non cortex, qui in corallis et titano-keratophytis occurrit, eodem modo alienum ab his rebus corpus ſit: et ideo an hæc corallia et keratophyta diverſa plane origine et alimento, quam ab hoc cortice fruantur? Ut in primo opuſculorum meorum ſubſecivorum faſciculo jam demonſtrare conatus ſum.

EXPLICATIO TABULÆ.

FIG. I. Repræſentat ramulum zoophyti, quod appellatur *corallina abietis forma*.

Circa hunc ramulum à menſe Septembri 1758 ad Februarium 1759 ſervatum, interea circumcreverat aſper è ſordibus cortex: quinque menſibus ad minimum ſemel quotidie recens aqua marina, priori abjecta, adfuſa fuit; et licet non multo increverit,

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(*n*) Vide Opuſcula mea Subſeciva, I. p. 27. et plura ſimilia apud Röſelium, loco citato, p. 605.

creverit, variis in locis laterales tamen emiſit ramulos, omnes polypis obſeſſos.

Fig. II. Eundem ramum lente inſpectum exhibet.

Litteræ ambarum figurarum rebus iiſdem ſunt ad ſcriptæ.

A. Truncus ſertulariæ, qui oſtracodermati inſedit.

B. Aliquot ramuli laterales, dum corallinam ſervavi, hinc inde emiſſi, et ab initio polypis obſeſſi.

C. Ramuli apex, qui recens omnino purus eſt, nec ſordibus aut polypis infectus.

D. Major polyporum ſpecies, principium zoophyti, quod *corallina tubularia* appellatur, parum increſcens.

E. Minima polyporum, ſive *ſertulariæ polypinæ*, Linn. 10. ſpecies deſcripta, et auctâ magnitudine delineata in primo meo opuſculo ſubſecivo, Tab. iii. fig. 1, A, B, C. Hujus ſpecies ſexcenta erant, adeo ut continua contractione et motu ſæpe conſpectum obfuſcarent.

Fig. III. Eſt apex C duarum priorum figurarum microſcopio inſpectus.

A. Locus ubi à trunco divulſus.

B. Duo ramuli laterales cum polypis ex illis, tamquam cellulis, prodeuntibus. Et

d. Brachia explicantibus.

e. Cellulæ, in quibus polypi contractis brachiis ſe penitus abſcondunt, tumque inſtar maculæ albæ apparent.

XXII. *An*

XXII. *An Account of an uncommon Phænomenon in* Dorſetſhire: *In a Letter from* John Stephens, *M.A. to* Emanuel Mendes da Coſta, *F. R. S.*

SIR,

Read April 9, 1761.

AS no eſſay, however imperfect, which tends to illuſtrate the operations of nature, can be unacceptable to the learned, I took the liberty to addreſs myſelf to you, in ſetting forth the following ſhort, but juſt account of a phænomenon obſerved in our own country, and, as far as I can recollect, not hitherto deſcribed.

In the month of Auguſt 1751, the air having been for ſome time remarkably hot and dry, was changed of a ſudden by a heavy fall of rain, and a high ſouth-weſt wind; the cliffs near Charmouth, in the weſtern part of Dorſetſhire, preſently after this alteration of the atmoſphere, began to ſmoke, and ſoon after they burned, with a viſible though a ſubtil flame, for ſeveral days ſucceſſively; and continued to ſmoke, and ſometimes to burn, at intervals, till the approach of winter: nay, ever ſince that time, eſpecially after any great fall of rain, thunder and lightning, or a high ſouth-weſt wind (which drives the ſea with great violence againſt the cliffs, and beats off large pieces of them), the cliffs continue to ſmoke, and ſometimes to burn with a viſible flame; which, during the ſummer months, is frequently obſerved in the night-time. On examining theſe cliffs, in the year 1759, I diſcovered a great quantity of pyrites, not in any regular ſtrata, but interſperſed in large maſſes through the earth, and which proved to be martial; of marcaſites, which yielded near one tenth part of common ſulphur; of cornua ammonis of different ſizes, and other ſhells, but of the bivalve claſs, which were cruſted over, and as it were mineralized, with the pyritical matter; of belemnites, alſo cruſted over with the like ſubſtance: and the cliffs, for near two miles long, and from the ſurface, to 35 or 40 feet deep, even to the rocks at high-water mark, were one bed of a dark coloured loam, ſtrongly charged with bitumen. Moreover, I found alſo a dark coloured ſubſtance, reſembling coal-cinder; ſome of which being powdered, and waſhed in diſtilled rain-water, upon filtrating the water, and evaporating it ſlowly to a pellicule, its ſalts ſhoot into fine cryſtals, and appear to be no more than a martial vitriol: one ounce of this cinder-like ſubſtance yields one drachm of ſalt. I gathered up about one hundred pounds weight of the different kinds of thoſe pyritæ, marcaſites, &c. which were laid in a heap, expoſed to the air, and every day ſprinkled with water: the conſequence was, that, in about ten days time, they grew hot, ſoon after caught fire, burned for ſeveral hours, and fell into duſt. Hence, therefore, it is imagined, that theſe martial and ſulphurous foſſils, by being expoſed to the air and wet, by being agitated by the beating of the ſea, and, if I may uſe the expreſſion, by being electrified by the ſubtil flame of the lightning, take fire, which is favoured by the bituminous particles contained in the loam, and burn till all their phlogiſton is conſumed, and their iron, or martial earth, is diſſolved in the acid of ſulphur;

which conſtitutes the martial vitriol, found to be near the one eighth part of this cinder-like matter.

When the cliffs were obſerved to burn in the night-time, the flame was plainly perceived by a ſpectator at a diſtance; but, when he drew near to the place, ſeemingly on fire, he could perceive a ſmoke, but no flame. In the day-time, nothing but a ſmoke was perceived, except the ſun ſhined, when the cliffs appeared, at a diſtance, as if they were covered with pieces of glaſs, which reflected the ſun's meridional rays; but, upon drawing near to the places, where theſe luminous appearances were perceived, they diſappeared, and the cliffs ſeemed to be covered with ſmoke, which ſtunk of a bituminous and ſulphureous matter.

I have alſo been an eye-witneſs of the ſame kind of flame ariſing from the Lodes in Cornwall, eſpecially ſuch, as contained a great quantity of mundic and martial pyrites. Three times I have ſeen this flame ariſe from the earth in the night, and once in the middle of the day. In the night, a perſon, ſtanding at a little diſtance, would imagine, that the place was all on fire, and even on drawing near the ſame, he perceives himſelf ſurrounded with flame, but is not hurt; and in four or five minutes time, he perceives this flame to decreaſe, and fall into the earth. In the day-time, the flame is of a different colour, and not much unlike the flame, which ariſes from a furnace. There are ſeveral mines diſcovered in this county by theſe mineral fires, where there were no ſymptoms of ſuch mines before: but it is generally obſerved, that they abound with marcaſite and pyrites. Moreover, theſe mineral flames, ariſing from ignited pyrites, are frequently diſcovered in the bottom of mines and coal-pits; and are often detrimental, and ſometimes deſtructive, to the miners; which made the late learned Dr. Woodward, and others, imagine, that they were vapours ariſing from an abyſs.

From what has been ſaid therefore, we may, in my humble opinion, draw the following concluſions.

1. That all ſubterraneous fires, even thoſe of Hecla, Veſuvius, and Ætna, together with thoſe obſerved in the mines and coal-pits, are cauſed by the heat and fixing of pyrites and marcaſites.

2. That the waters of our hot baths derive their heat from paſſing over a bed of ignited pyrites. Indeed the ſolid contents of thoſe waters do evidently prove this aſſertion being nothing more than ſuch particles of the pyrites as are ſoluble in water.

3. That theſe mineral flames will be more or leſs ſubtil, according to the minuteneſs of the particles of the combuſtible matter, and the quantity of phlogiſton, which they contain.

4. That the convulſive motions and tremblings of the earth are cauſed by the heat of the burning pyrites expanding the air contained in its bowels. This is clearly proved, by their cauſing, immediately after, an eruption of the earth, which generally diſcharges a dark coloured cinder-like and frothy matter. And,

5. That thoſe places, where the earth contains the greateſt quantity of pyrites and marcaſites, will be moſt liable to theſe convulſive motions and tremblings, no other natural cauſe contradictory.

However, I ſhall, with great reſpect, ſubmit theſe obſervations to the conſideration of the Preſident and

Fellows of the Royal Society, to whom, Sir, if you will be ſo kind as to communicate them, you will greatly oblige,

SIR,

Your very humble ſervant, &c.

Woodſtock-Street,
April 4, 1761.

John Stephens.

XXIII. *Additional Obſervations upon ſome Plates of white Glaſs found at* Herculaneum: *In a Letter to* Charles Morton, *M. D. R. S. S. By* J. Nixon, *A. M. and F. R. S.*

Dear Sir,

Read April 9, 1761.

IN a paper, which I had the honour to preſent to this learned Society about [*] two years ago, I offered my thoughts upon ſome plates of white glaſs found in the ruins of Herculaneum. I now beg leave to add ſome more obſervations, with a view partly to explain and ſupport what I then delivered, and partly to communicate ſuch new informations, as I have ſince received, relating to the ſame ſubject.

I obſerved [†], upon the authorities produced by Monſ. Renaudot [‡], that glaſs plates were not applied for magnifying objects in optical experiments, till the beginning of the thirteenth century: but, upon reviewing his diſſertation, I find he ſinks the antiquity of that uſage a century lower than this. That learned writer adds further, "That with regard to this queſtion, whether the antients made their aſtronomical obſervations without teleſcopes, the affirmative is looked upon as certain; becauſe, if this invention had ever been known before, there is all imaginable reaſon to believe, that the utility, which would reſult from it, not only in aſtronomy, but for ſeveral other purpoſes, would have prevented its being afterwards loſt." Monſ. Renaudot declines entering into this controverſy; but obſerves, that Mabillon mentions a manuſcript he ſaw in an abbey in the dioceſe of Freiſingen, wherein Ptolemy was repreſented obſerving the ſtars with a * tube, like our modern perſpective-glaſſes. This manuſcript is ſaid to have been written in the beginning of the thirteenth century; which date (ſays Monſ. Renaudot) is the more remarkable, becauſe plain ſpectacles, which ſhould ſeem likely, in the nature of things, to have been invented firſt, do not appear to have been known till a hundred years after. Then, having produced the evidences, which prove, that this latter diſcovery was made about the time above-mentioned, he concludes with ſaying, "that

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[*] Phil. Tranſ. Vol. L. Part II.

[†] Ibid. p. 602.

[‡] Mem. de l'Acad. des Inſcript. Vol. I.

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* Mabillon does not mention, that the tube had glaſſes; neither indeed was that circumſtance eaſily diſcoverable. Perhaps ſuch tubes were then uſed only to preſerve and direct the ſight, or to render it more diſtinct, by ſingling out the particular object looked at, and ſhutting out all the rays reflected from others, whoſe proximity might have rendered the image leſs preciſe.

" we have nothing of this nature with regard to te-" leſcopes."

The reaſon of my enlarging upon this article is a paſſage I have lately met in that learned antiquary, Mr. Rowland, which may ſeem to contradict the obſervation produced above. This * author alleges the authority of Hecatæus (apud Diod. Sic. tom. i. p. 159. Ed. Weſſel.) for ſaying, that the Hyperborei, who inhabited an iſland in the Northern ocean, oppoſite to the Celtæ, " could (as if they had the uſe " of teleſcopes) ſhow the moon very near them, and " diſcover therein mountains, and heaps of rocks, " which that inſtrument only can diſcover." That we may diſtinguiſh how far Hecatæus is concerned in this paſſage, it will be proper to give a literal tranſlation of it from the original; viz. " They ſay " further, that the moon, viewed from this iſland, " appears to be but at a very little diſtance from the " earth, and to have certain protuberances, like land, " viſible on her ſurface †." Now, it may be obſerved, in the firſt place, that this phænomenon, if real, may perhaps be explained by the refraction of the moon's rays in paſſing through the atmoſphere of the earth, which, in an iſland ſituated very far north, might be continually charged with an extraordinary quantity of vapours. Or further, as Hecatæus mentions it upon hearſay only, and ſubjoins ſome other circumſtances in the ſame chapter relating to this iſland, which are entirely of a fabulous caſt,

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* Mona Antiqua, p. 76.

† Φασὶ δὲ ϗ τὴν Σελήνην ἐκ ταύτης τῆς Νήσου φαίνεσθαι παντελῶς ὀλίγον ἀπέχουσαν τῆς Γῆς, ϗ τινας Ἐξοχὰς γεώδεις ἔχουσαν φανεράς.

we may juſtly queſtion the * truth of the fact; and, conſequently, ſhall not be obliged to maintain the neceſſary exiſtence of teleſcopes in thoſe times, in order to account for it.

As it appears †, that neither the lapis ſpecularis, nor glaſs, was uſed for windows before Seneca's time; and it cannot be ſuppoſed, that the Romans, a people of ſo refined a taſte in other inſtances, would ſuffer their apartments to be expoſed to the free entrance of winds, &c. it may be reaſonably aſked, What ſupplied the place of thoſe materials before? To ſatisfy this enquiry, it is to be obſerved, that ſeveral other materials are mentioned by antient writers, as ſerving the purpoſe before us; ſuch as thin hides, or ‡ ſkins, like our parchment, mentioned by Philoponus. Pliny likewiſe informs us, that the horns of the urus being cut into thin laminæ were || tranſparent, and ſupplied, in ſome meaſure, the uſe of our lanthorns; and we may probably conclude, from the analogy of things, that they ſerved for window-lights alſo; eſpecially, as we meet with windows made of horn (corneum ſpecular) in Tertullian, who wrote within leſs than two hundred years after Pliny.

To theſe, we may add the vela, made of § hair-cloth, or pieces of hides ||||, which Pitiſcus (upon the

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* Vide Weſſelium, not. in loc.

† Phil. Tranſ. Vol. L. Part II. p. 605.

‡ Apud Salm. Exerc. Plin. T. ii. p. 1095. Ed. Par.

|| Plin. Nat. Hiſt. L. xi. c. 37. In laminas ſecta tranſlucent atque etiam lumen incluſum latius fundunt. Apud Salmaſ. Plin. Ex. T. i. p. 260.

§ Vela cilicia. Ulpian apud Le Antichita di Ercolano eſpoſte, p. 268.

|||| Fabretti. Ibid. p. 256. The makers of theſe vela, Σκηνοποιοί. Act. 18. 3. ibid.

authority of Ulpian) ſays, were in uſe before the invention of windows of the lapis ſpecularis, or * glaſs. Ulpian indeed, in the paſſage Pitiſcus refers to, only mentions them as ſubſiſting together with the † latter: but it ſeems obvious to conclude, that the vela, being an invention leſs perfect and commodious, were prior in time to the ſpecularia, which are to be regarded as a ſubſequent improvement of the former. Notwithſtanding this, the vela ſtill continued in queſtion, even after the introduction of window-fences of ſtone or glaſs, and ſerved as canopies, or ‡ umbrellas, to keep the ſun from places expoſed to the open air; as the others ſecured the inner parts of the houſe from cold, &c.

I took notice ‖ of the natural connection there ſeemed to ſubſiſt between the uſing of plates of glaſs for adorning the inſide of apartments in antient times, and the employing them for introducing light into thoſe apartments. This obſervation has been ſupported by a letter I received from my learned correſpondent, Abbate Venuti, at Rome, dated December 30, 1759, wherein he informs me, that he had lately read, in ſome anecdotes of Cardinal Maximi, " That as they were digging among the ruins on " mount Cælius, in the laſt century, they found a " room belonging to an antique dwelling-houſe, that

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* Pitiſcus, Tit. Specular.

† Specularia et vela, quæ frigoris cauſâ et imbrium in domo ſunt. Ibid.

‡ Specularia-vela, quæ frigoris, vel umbræ cauſâ, in domo ſunt. Ulpian apud Le Antich. See theſe vela exhibited, Tavol. vi. & 49. ibid.

‖ Phil. Tranſ. Vol. L. Part II. p. 606.

" had all its ſides within ornamented with plates of " glaſs, ſome of them tinged with various colours, " others of their own natural hue, which was duſky, " occaſioned by the thickneſs of the maſs, of which " they conſiſted *. There were likewiſe in the ſame " apartment, window-frames compoſed of marble, " and glazed with laminæ of glaſs." But as the Abbate did not take upon himſelf to aſcertain the real age of this building, I ſhall not pretend to lay any greater ſtreſs upon this diſcovery, than I did upon the obſervation, for the ſake of which I produced it, for proving the point I had then in view, viz. that the uſage of glaſs for windows was (probably) nearly of the ſame antiquity with that of adorning houſes with it.

I informed the Society †, that I had not been able to trace up the conſtructing of windows with plates of glaſs, ſuch as theſe found at Herculaneum, higher than two hundred years ſhort of the overthrow of that city: but, ſome time after, a paſſage in Baronius was ſuggeſted to me, which ſeemed to carry the antiquity of this practice much higher, even to the 42d year of the Chriſtian æra. It was a quotation ‡ from Philo Judæus, wherein he gives an account of C. Ca-

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[*] Nam cum laminæ craſſioris eſſent molis, colorem opacum nigrantemque reddebant. Venuti. This would be the effect of the antient glaſs, if it was of a coarſer compoſition than ours: and that it was ſo in fact, a very eminent critic, both in ſacred and profane literature, thinks, may be collected from St. Paul's words, 1 Cor. xiii. 12. " Now we ſee, but through a glaſs " darkly."

[†] Philoſ. Tranſ. Vol. L. Part II. p. 608.

[‡] Baron. Annal. Eccleſ. T. i. A. C. 42. p. 335. Col. Agrip. 1621.

ligula's reception of the Jewiſh deputies. "When
"(ſays he) we had entered upon our harangue, the
"Emperor perceiving, that ſome things of no ſmall
"weight were urged, and that others no leſs ſtrong
"were likely to be alleged, he broke off the audience,
"and hurried away, with great precipitation,
"into a ſpacious hall: there walking * about, he
"commanded the windows to be ſhut on every ſide,
"conſiſting of white glaſs, reſembling plates of the
"lapis ſpecularis, which admit the light, but exclude
"the wind and the ſun."

This authority indeed, if genuine, would have fully anſwered my purpoſe; but, upon conſulting the text of Philo, I was fully convinced, that the Cardinal's tranſlation of the latter part of this paſſage, which alone affects the preſent inquiry, was directly contrary to the original; which imports, that the windows in the imperial apartment conſiſted of laminæ of ſtone, almoſt as tranſparent as glaſs †.

I cannot leave this paſſage, without taking notice of that concluſion of it, viz. "That the windows of
"the lapis ſpecularis admitted the light, but excluded
"the violent heat of the ſun." This ſeems to prove,

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* Obambulanſque juſſit claudi feneſtras vitro candido ſimili lapidibus ſpecularibus, quibus lux admittitur, ventus et ſol excluditur. This verſion of Baronius is the ſame verbatim with that in the editions of Geneva 1613, Lut. Par. 1640, and Francf. 1691.

† Προςάττει τὰς ἐν κύκλῳ θυρίδας ἀναληφθῆναι τοῖς ὑάλῳ λευκῇ διαφανέσι παραπλησίως λίθοις, οἳ τὸ μὲν φῶς οὐκ ἐμποδίζουσιν, ἄνεμον δὲ εἴργουσι, ϗ τὸν ἀπ' ἡλίου φλογμόν. Ed. Lut. 1640 & Franc. 1691. Since the writing of this, Dr. Birch has informed me, that Dr. Mangey has tranſlated this paſſage agreeably to my idea, viz. Lapidibus haud minus pellucidis quam vitro candido.

that the ſpecularia in Martial were made of the ſame materials, if this reading adopted by Salmaſius, &c. is to be followed; viz.

Specularia puras
Admittunt luces, et ſine ſole diem.
L. viii. Epig. 14.

But other copies have it

Specularia puros
Admittunt ſoles, et ſine fæce diem *.

This reading is eſpouſed by Colleſſus, the Dauphin editor, who further explains (puros) by (nitidos); and yet, in his notes, tells us, that theſe ſpecularia were of ſtone or talc; which they could not have been, conſiſtently with Philo's account, but muſt have been of glaſs; and conſequently, we ſhould have an evidence in Martial for the uſage of glaſs in windows, as early as the firſt century: for that poet lived in Rome from A.C. 71 to 100.

But perhaps theſe (ſeemingly) contradictory readings of this paſſage may be reconciled, as to their ſenſe, by interpreting (puras luces) in the one, and (puros ſoles) in the other, to mean the mild light and warmth of the ſun, which remained after the greater part of its rays had been either reflected by the exterior ſurface, or abſorbed within the interior pores of the ſtone; or, as Milton expreſſes it,

The ſun ſhorn of his beams.

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* Ed. Ingolſt. 1602. Pitiſcus Specular. &c.

Upon this hypotheſis, ſine fæce will ſignify the excluſion, not of the rain, duſt, &c. as it is explained by the commentators, who follow this reading; but that of the groſs body of the ſun's rays; and ſo will coincide with ſine ſole diem, in the other copies.

As I quoted * Lactantius (De Officio Dei, c. viii.) to prove the uſe of glaſs in windows in his time, viz. the third century, I hold myſelf obliged to take notice of the cenſure, which Cortius and Longolius paſs upon this father, and which is as far from being candid, as the authorities they appeal to are from proving it true. Theſe gentlemen, in their notes on Pliny (L. ii. Ep. 17.), boldly pronounce the father miſtaken (peccavit Lactantius) with regard to the paſſage I produced from him: and they ſupport this charge, by referring to Lipſius on Seneca de Prov. C. iv. & Epiſt. 90. and to Pliny Hiſt. Nat. L. xxxvi. c. 26. Now, whoever conſults Lipſius on the places here referred to by theſe editors, will find nothing therein, but obſervations relating to the lapis ſpecularis, viz. the reaſon of its name; the countries where it was found; its uſe in window-fences, for dining-rooms, bed-chambers, baths, porticos, and even in orchards and gardens. This is what nobody ever denied, and what even Lactantius himſelf intimates, in the † paſſage before us. How, therefore, this can affect that father's teſtimony, relating to the uſe of glaſs in windows, exceeds my imagination to conceive. And

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* Phil. Tranſ. Vol. L. Part II. p. 608.

† Manifeſtius eſt, mentem eſſe, quæ ea, quæ ſunt oppoſita, tranſpiciat, quaſi per feneſtras lucente vitro, aut lapide ſpeculari obductas.

as for Pliny, I ſuppoſe it will readily be allowed me, that no writer, how reſpectable ſoever his authority may be, can poſſibly prove another, who lived two hundred years after him, miſtaken, when he alludes to the practice of his own times.

As I hope the evidence is now undeniable, which I produced in my diſſertation, to prove the uſe of glaſs in windows to have been as early as the third century (not to mention the probable reaſons there offered to ſhew, that it might have ſubſiſted ſome ages before), it may not be unacceptable to the curious in antiquity, to obſerve the ſlow progreſs this very commodious invention made in travelling towards the weſt; ſince it appears, by our hiſtorians *, that it did not reach our iſland till the ſeventh century; when it was brought hither from France, either by Benedict abbot of Winal, or Wilfrid archbiſhop of York; as † lanthorns of horn were introduced by King Alfred, about the ſame time, viz. 680.

Having now propoſed all I had to offer, relating to the ſeveral uſes of plates of glaſs, already mentioned in my eſſay, I beg the Society's indulgence to permit me to ſubjoin two others, which I have met with ſince that communication.

The firſt of theſe was ſuggeſted to me by my (late) worthy friend Smart Lethieullier, Eſq; who, laſt winter at Bath, informed me, that he had in his collection an urn, of a quadrangular figure, which had been divided into two equal parts by a plate of glaſs, the veſtiges of which were ſtill remaining. He was of opinion, that the cells made by this partition contained the remains of ſome pair, eminent either for their conjugal affection, or ſome of the other connections of ſocial life. This conjecture, highly probable in itſelf, is farther confirmed by ſimilar examples in antiquity. Thus we find in Montfaucon * the figure of a ſquare urn, wherein were contained the aſhes of a man and his wife, as appears by the inſcription upon it. Another urn is repreſented (Plate LVII.), which held the aſhes of a mother and her daughter. To which we may add a third (Plate LV.), covered with a ſquare flat tablet of ſtone, on which were three inſcriptions; ſignifying, that the remains of three perſons, whoſe relation to each other is not ſpecified, were incloſed therein.

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* Simon Dunelm. Hiſt. Ang. Script. p. 92. Stubbs Act. Pont. Ebor. - Hiſt. Ang. Script.

† Staveſly's Hiſt. of Churches, p. 103.

The other inſtance was tranſmitted to me by the Abbate Venuti, in a letter from Rome, dated September 27, 1759. viz. " That, in digging up ſome " ruins in that city a few years ago, there was found " an antient picture painted on marble, and covered " with a plate of white glaſs, like thoſe uſed in our " times for that purpoſe, only ſomewhat thicker. " The picture expreſſed a lady's head, and was of a " very elegant compoſition." From this laſt circumſtance, the Abbate infers, " that it could not be the production of any later age;" meaning (I preſume) any period between the decay of good painting among the antients, and the revival of it among

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* Antiq. Expliq. Vol. V. p. 1. Pl. 34. Ed. Par.

the moderns. He further aſſures me, that he ſaw this picture, which (together with its cover) was depoſited in the cabinet of the Marquis Capponi at Rome.

The circumſtance of this piece being painted on marble, naturally leads our thoughts up to the age of the fragments of glaſs, which occaſioned my diſſertation, viz. to the overthrow of Herculaneum, out of whoſe ruins four pictures (among many others) have been found painted on the ſame materials. There is a paſſage in Pliny *, which has been thought to carry up this manner of painting as high as to the times of Claudius, who began to reign A. C. 41. But I am humbly of opinion, that lapidem pingere, in this place, does not mean painting on ſtone or marble, but only the ſtaining them with artificial colours; as the remaining part of the ſentence relates to the inlaying of pieces of marble of various tints, where the original veins were defective, either in variety or beauty: not that I think it at all improbable, at the ſame time, that this ſpecies of painting might be as antient as the epocha mentioned above, viz. the reign of Claudius; becauſe it actually ſubſiſted in the time of Pliny, which muſt reach up to that æra; for the four paintings referred to in the beginning of this paragraph, as done in the ſame manner, were found in the ruins of a city (viz.

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* Cæpimus et lapidem pingere. Hoc Claudii principatu inventum. Neronis vero maculas, quæ non eſſent, in cruſtis inſerendo unitatem variare, ut ovatus eſſet Numidicus, ut purpurâ diſtingueretur Sinnadicus, qualiter illos, naſci optarent deliciæ. Hiſt. Nat. Lib. xxxv. c. 1.

Herculaneum), in whoſe cataſtrophe that writer loſt his life.

I am,

S I R,

Your moſt obedient,

humble ſervant,

London, Feb. 3, 1761. J. Nixon.

XXIV. *A Deſcription of the Cepphus: In a Letter from* D. Lyſons, *M. D. to* Robert More, *Eſq*; *F. R. S.*

S I R, All-ſouls Coll. Oct. 17, 1760.

Read April 16, 1761.

TO ſave you the trouble of taking an account of the bird I ſent you, I have now taken the liberty to trouble you with the incloſed deſcription, which is pretty near, though, perhaps, not quite exact. Ray, in Willoughby's ornithology, ſays, this bird is yet to us unknown; and takes his deſcription of it from Aldrovandus, who ſays, it was not deſcribed by any author before his time, that he knows of.

The bird before us is, I think, the cepphus of Aldrovandus, though it does not agree in all points: perhaps, that he ſaw, might be a male, this a female. In his, the ſides of the mandibles were of a duſky red, in this not. The eyes of his were partly red, which I did not obſerve in this. Round the eyes in his was a whitiſh circle, in this a variegated ſemicircle. The legs and ſhanks in his greeniſh, in this of a dilute blue. The feet, and membranes connecting the toes, in his were duſky, in this partly black.

Some authors have ſuppoſed the cepphus of the antients to be the fulica, or coot. This Aldrovandus confutes, by many arguments, one of which is ſufficient. He quotes a paſſage from Varrinus, where the cepphus of Ariſtotle is mentioned as a ſea bird, and having the claws connected by a membrane, which the coot has not; neither is the coot a ſea bird, being often found, in great numbers, upon lakes and ſtanding waters.

Aldrovandus reckons his cepphus as a ſpecies of the larus; which is denied by Pierius Valerianus, upon the ſtrength of an argument, which, I think, tends ſtrongly to prove it. "Cepphus enim, inquit, "ad cibum quæcunque veſcatur, ut etiam maritima "ſit ſpuma contentus; larus vero inter voraciſſimas "numeratur, omnivorumque animal eſſe fertur." The cepphus is ſaid to be a very active bird, always flying about in queſt of its prey, which is bits of fleſh, or fiſh, left by other fiſh of the voracious kind, or, in ſhort, any kind of food it meets with ſwimming upon the ſurface of the water. Now, ſuch ſubſtances as ſwim upon the ſurface of the water are not ſo likely to be met with any where, as amongſt the ſcum and froth of the ſea, driven together by the wind. This bird therefore dipping ſo frequently into the ſpume of the ſea, is probably for the food ſwimming amongſt it, rather than to feed upon the ſpume itſelf. After this character given of the omnivorous cepphus, it is ſomething extraordinary, that Valerianus ſhould refuſe ranging it in the claſs of lari, becauſe it was not ſufficiently voracious.

Another reaſon, why this bird may be ſuppoſed to be the true cepphus, is the ſimple manner, in which it was taken. In a field adjoining to Oxford, called the Parks, was ſome radiſh, or ſome ſuch ſort of ſeed, covered with old nets, to keep off the ſmall birds. In theſe nets was the bird entangled, and taken. Its being ſo far in land was poſſibly occaſioned by the late ſtormy weather.

A Deſcription of Aldrovandus's Cepphus.

It weighed eleven ounces. Its meaſure was, from the tip of the bill to the end of the tail, 15 inches. From the tip of one wing to the tip of the other, when extended, 39 inches. Round the body, where the wings are ſet on, 11 inches. Round the body, where the legs are ſet on, 8 inches and an half. From the angle of the mouth to the point of the beak, an inch and three quarters.

The anterior part of the upper mandible is of a lead colour, and rough, reſembling horn in appearance. The point of the bill is black, crooked at the end, ſmooth, and of a harder ſubſtance than the other part. The inferior mandible is, in its anterior part, of a lead colour, the point black, but all ſmooth. Where the two ſides of the lower chap meet in an acute angle underneath, is a ſmall prominence, or knob. Its noſtril extends almoſt the length of the beak, the aperture being wideſt towards the point; as it approaches the head, it is almoſt cloſed up.

Its neck is ſhort; its body in ſhape much like a wild duck. The throat, neck, breaſt, and belly, are of a very duſky colour, variegated with white, yellow, or brown, intermixed; under the throat, about the eyes, upon the breaſt and belly, being more variegated than between the throat and breaſt. Below the vent, the feathers under the tail are marked with bright yellow tranſverſe lines.

In the wing are twenty-eight black quill feathers, the ten outermoſt of which are tipped with brown; the feathers incumbent upon the quill feathers are alſo tipped with brown; ſo that, upon the wing extended, four beautiful tranſverſe lines appear. The ſuperior part of the wing, and the ſhort feathers under the wing, are beautifully variegated with a bright brown or yellow; on the under ſide, the quill feathers are of a duſky colour, and ſhining, the five outermoſt being partly white.

The head is ſmall and ſlender; of a dark colour, variegated with light brown, as is the upper part of the neck. The lower part of the neck, and the back, are of a dark duſky colour.

The tail has twelve feathers, the ſhorteſt of which are 4 inches, and the middle, which are the longeſt, not above 4 and an half. The ends of them all are black; but part of them towards the rump, are white about one fourth of their length. Upon the tail, on each ſide, are a few feathers incumbent, marked with tranſverſe bright brown lines.

From the joint between the leg and thigh to the end of the longeſt claw, is 3 inches. The legs are of a bluiſh lead colour. The back claw is ſmall, and black; the other three claws are connected by a membrane, the extremity of which is black; its anterior part white, or lightly tinged with yellow; the innermoſt claw is of the ſame lead colour with the leg, to the laſt joint; the middle claw only to the firſt joint; and the outer claw has a very little lead colour upon it, but not to the firſt joint. The extremities of all the claws are black. The nails, which are ſmall, are all black. The middle nail has a keen broad edge on its anterior ſide.

After ſo long an account of this bird, as I have troubled you with, perhaps you may be ready to conclude, with the ſame line, that Aldrovandus ends his obſervations upon the cepphus,

> Parturiunt montes, naſcitur ridiculus mus.

But as my intention is good, I hope that will be accepted as an excuſe, for the great pains I have put you to in reading ſo long a letter; and that I may be permitted to ſubſcribe myſelf,

SIR,

Your moſt obedient

humble ſervant,

D. Lyſons.

XXV. *An Extract of the Regiſter of the Pariſh of* Holy-Croſs *in* Salop, *from* Michaelmas 1750 *to* Michaelmas 1760: *Communicated by* Robert More, *Eſq;* *F. R. S.*

Read April 16, 1761.

		1751.	1752.	1753.	1754.	1755.	1756.	1757.	1758.	1759.	1760.	Total.	
Baptized,	Males.	19	16	9	22	23	15	14	11	25	14	168	331
	Females.	18	22	18	12	16	17	18	12	15	15	163	
Buried,	Males.	19	16	16	15	6	16	11	12	16	10	137	290
	Females.	11	12	14	20	10	11	11	24	23	17	153	
												Increaſe.	41.

	1751.	1752.	1753.	1754.	1755.	1756.	1757.	1758.	1759.	1760.	Total.
Under a month old.	2	—	3	2	3	2	—	1	3	1	17
From a month to a year.	1	2	3	5	1	1	4	5	1	4	27
From 1 to 2	2	2	1	1	1	—	2	3	2	1	15
2 — 5	5	3	2	2	—	2	1	8	3	2	28
5 — 10	2	3	3	2	1	3	1	3	2	3	23
10 — 15	1	1	1	—	—	2	—	—	1	—	6
15 — 20	—	—	—	2	—	2	—	—	3	—	7
20 — 25	1	1	3	2	—	3	1	1	4	2	18
25 — 30	2	1	—	3	—	2	—	1	1	1	11
30 — 35	—	4	1	1	—	—	—	1	1	1	9
35 — 40	2	1	1	2	—	2	1	—	2	—	11
40 — 45	—	—	—	2	1	—	1	1	6	2	13
45 — 50	1	1	2	1	—	1	2	—	—	—	8
50 — 55	1	—	2	1	—	2	2	2	—	—	10
55 — 60	2	1	1	1	1	2	—	2	1	2	13
60 — 65	—	2	—	3	2	1	—	2	2	1	13
65 — 70	2	1	—	1	1	1	2	2	3	2	16
70 — 75	3	—	1	—	2	—	1	2	—	1	10
75 — 80	1	2	2	—	1	1	—	1	1	1	10
80 — 85	1	3	2	1	1	—	3	1	1	2	15
85 — 90	1	—	2	2	—	—	1	—	1	1	8
92 and 96	—	—	—	—	1	—	—	1	—	—	2

There remains alive,

From 70 to 75	Males Females	5 16	21	From 80 to 85	Males Females	1 7	8
From 75 to 80	Males Females	1 6	11	From 85 to 90	Males Females	1 6	7

Nº of houſes, or families	235	Nº of perſons in 1755	1049
Houſes paying window tax	77	Ditto 1760	1048
Nº of acres there is on waſte	1700	Void houſes	4

Apoplexy	2	Convulſions	9	Palſey	1
Cancer	2	Dropſy	10	Quinſy	1
Childbed	4	Fever	39	Small-pox	33
Chincough	9	Jaundice	3	Stone	1
Cholic	1	Impoſtume	2	Teeth	1
Conſumption	47	Meazles	4		

XXVI. *An Account of the Earthquake at* Liſbon, 31*ſt* March 1761: *In a Letter from thence, dated the 2d* April 1761, *to* Joſeph Salvador, *Eſq; F. R. S.*

Read April 23, 1761.

THE earthquake happened the 31ſt laſt month, preciſely at twelve o'clock, and laſted full five minutes, with a ſmart and equal vibration. It exceeded all the others, except that of the firſt November 1755. Thank God, it was attended with no other conſequences, but that of alarming the inhabitants, throwing down ſome ruins, and rending ſome houſes. About an hour and a quarter afterwards, the ſea began to flow and ebb, about eight fleet perpendicular, every ſix minutes, and continued till night. Some ſmall ſhocks were felt before and ſince, but of no moment;

every body ſeems at eaſe, and things go on in their uſual channel.

Mr. Salvador has received many other letters, which ſeverally confirm theſe particulars.

XXVII. *Another Account of the ſame Earthquake: In a Letter from Mr.* Molloy, *dated there* April 3, 1761, *to* Keane Fitzgerald, *Eſq; F. R. S.*

Read April 23, 1761.

ON the 31ſt ult. at twelve o'clock, we had a moſt dreadful violent ſhock of an earthquake, that held conſtant for five minutes, as near as I can judge. I was up two pair of ſtairs, at a friend's houſe, when it began, and expected to have been buried in the ruins. The ſhock, as it appeared to me, ſeemed to ſpring from the bowels of the earth, and the motion to be directly up and down. It is the general opinion, that if it had run from weſt to eaſt, or from any quarter of the globe to the other, as the great one the firſt of November 1755 did, there would not have been a houſe left ſtanding in this unfortunate place, as all the gentlemen that reſide here ſay, it was more ſevere and conſtant for the time than the former. Many buildings have tumbled down, but few people were killed; ſome have died through fear, and about 270 felons, in the confuſion it occaſioned, got out of gaol, who, it is feared, will commit great exceſſes, before they are taken again. Orders were iſſued by S. J. de Carvalho, that, on pain of death, no perſon ſhould leave the city by land, nor go on board any ſhip, or boat, without a licence from an office, appointed for that purpoſe.

The agitation of the ſea was very great, during the time of the tremor; and, for ſome hours after it, the waters ebbed and flowed many feet perpendicularly, ſeveral times in the ſpace of every ſix minutes. Ships at anchor in the river, though riding in ſome fathoms of water, were left dry at ſome intervals. In ſhort, nothing but terror and deſolation appeared in every countenance; the earth groaned in ſo dreadful a manner, that we expected every moment it would open, and ſwallow this place, and all its inhabitants. We have had ſeveral ſlight ſhocks ſince, and one this morning, about two o'clock, which was very ſevere; our houſe ſhook like a bulruſh. There was another more ſlight about five.

XXVIII. *A further Account of the Caſe of* William Carey, *whoſe Muſcles began to be oſſified: In a Letter to the Right Honourable the Lord* Cadogan, *F. R. S. from the Rev.* William Henry, *D. D. F. R. S.*

My Lord,

Read April 30, 1761.

I Should have long before this time acknowleged your Lordſhip's Letter, of the 19th of February, and your inquiries concerning William Carey, the oſſified young man; but as your letter came to me in the country, where I was at a conſiderable diſtance from all opportunities of making a full and ſatisfactory inquiry, I judged, that it would be more acceptable to your Lordſhip, that I ſhould defer giving you trouble, until I could give you a ſatisfactory anſwer.

In March 1759, I had this young man brought up to Dublin, and admitted into Mercer's hoſpital. The phyſicians and ſurgeons put him under a ſalivation; and afterwards applied, to his arms and joints, mercurial plaiſters. The good effects of this proceſs, was the drying up the great diſcharge of humour, which he had at his elbows and wriſts, and an immediate check to the progreſs of the oſſification.

In June following, he was diſcharged from the hoſpital, being furniſhed with mercurial plaiſters, and directions. By the advice of the phyſicians, he went to his own place, near Ballyſhanon, on the weſtern ocean; and there, in purſuance of their directions, bathed in the ocean twice a day, during that whole ſummer and harveſt, and conſtantly rubbed his whole body and limbs over with the juice of the quercus marina, immediately after coming out of the ſea.

In conſequence of this courſe, he happily exchanged his ghaſtly hectic countenance, for an healthy and athletic complexion, which continued until March 1760.

About this time his cough returned, his ſores began to run, and the oſſification to return. In this diſtreſs, he came to me to Dublin. With ſome difficulty I got him admitted again into Mercer's hoſpital; where he continued for ſome months, and was again treated with mercurial medicines and applications,

tions, as before. After being diſcharged, he returned to his former courſe of bathing in the ocean, and anointing his body with the quercus marina.

This proceſs reſtored his health, and intirely ſtopped the progreſs of the oſſification. He alſo recovered the uſe of ſome of the oſſified joints, particularly of his wriſts and fingers; and his knees and legs grew ſo relaxed, by the diſſolution of the callus, that he was able to walk twenty miles in a day.

I feared, that his diſorder might return this ſpring, as it did in 1760; but it has not returned. That I might be the better certified, I wrote to Sir James Caldwell. The anſwer I received was, that he had been, a few days ago, at Caſtlecaldwell, and found himſelf ſo well and ſtrong, as to importune Sir James to admit him into his body of the Enniſkillen light-horſe. The poor man thinks the oſſification intirely ſtopped; yet, by the appearance of his arms and wriſts, he ſeems to be miſtaken. The firſt hardneſs ſtill continues; and all the muſcles from his elbow to the wriſt, ſeem to be one ſolid bone. It is very happy for him, that it has been hitherto ſtopped from proceeding any farther; and that, from his preſent ſtate of good health, there is reaſon to hope, that it will not increaſe. I am,

With all reſpect, and gratitude
for your many obligations,
Your Lordſhip's
most obedient
and moſt humble ſervant,
William Henry.

Kildare-Street in Dublin,
April 16, 1761.

XXIX. *A Deſcription of a new Thermometer and Barometer: In a Letter to the Right Honourable* George *Earl of* Macclesfield, *Preſident of the Royal Society, from* Keane Fitzgerald, *Eſq; F. R. S.*

My Lord,

Read May 7, 1761.

I Took the liberty of addreſſing a paper to your Lordſhip laſt year, with an account of an inſtrument, which was intended to anſwer, in ſome meaſure, the purpoſes of a thermometer and pyrometer. The degrees the index had pointed to, during the abſence of an obſerver, were marked by a pencil applied to it. But I found great inconvenience from the friction of the pencil, which muſt be ſtrong, or it does not mark diſtinctly; beſides the trouble of rubbing out the mark, every time a new obſervation was intended.

I muſt beg leave to trouble your Lordſhip with the deſcription of an inſtrument on the ſame principle, as a thermometer only, with regiſters to mark the leaſt variation that can happen during the abſence of an obſerver, which are ſet for any future obſervation, with the greateſt eaſe. As this inſtrument is, in part, like the former, I ſhall only mark the variations from it.

The firſt bar A is fixed at the upper end, by three ſcrews *b*, *b*, *b*, and joined at the lower end to the arm of the firſt lever, by a pin *c*, which paſſes through both. [*Vide Tab.* V.]

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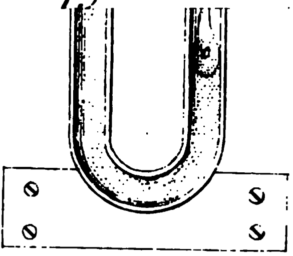
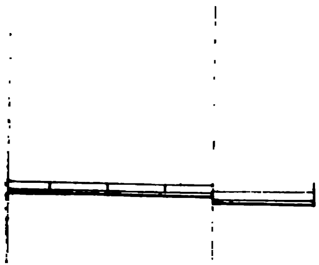
fig. 3.





J. Mynde sc.

The fourth bar *a* moves upon two ſmall pullies *d*, *d*, placed under it, and alſo a large pulley B, placed at the ſide of the bar, towards the upper end. In each of theſe pullies, there is a deep groove for the bar to paſs freely, without touching the ſides; and on each ſide of the groove of the pulley B, there is a channel cut for a thread to paſs, which is fixed to the fourth bar, by a hook at *f*, and has a weight ſuſpended at the other end. The thread placed in the inward channel, paſſes alſo over a ſmall pulley *e*, about ½ inch diameter, on the axis of which the index K is placed. The two weights *g* and *h*, ſuſpended to theſe threads, ſerve as a counterballance to the fourth bar, and keep it in contact with the pulley B, which turns with the bar as it moves.

Each of the levers is counterballanced by a weight *i*, at the end of a thread, which paſſes over a pulley *p*, placed above the lever, towards the end of its longer arm, and faſtened to it by a hook at *q*. In adjuſting theſe weights, it is neceſſary, that each lever ſhould preponderate a little towards the ſhorter arm, in order to keep that end cloſe to the bar placed on it. The counterballance weights of the fourth bar are ſo much lighter, as to allow a ſuperior gravity to the bar, ſufficient to turn the index and regiſters; by which means, all the levers bear the ſame way, whether the bars are contracted or expanded.

The axis of the ſmall pulley *l*, on which the index is placed, moves on friction wheels applied to each end. There are two regiſters, or ſlender hands, *k*, *k*, each of which is placed on a circle of braſs *l* and *m*, *l* about 2½ inches diameter, and *m* 2¼, placed a little more forward than *l*, ſo as to admit each to move freely, without touching the other. Theſe circles turn, each upon three friction pullies *n*, *n*, *n*, and *o*, *o*, *o*. The regiſters, which are very ſlender, are counterballanced by a ſmall weight placed on the oppoſite ſide of the circle, and moved by a pin, which paſſes through the index, and takes one along with it, as it moves one way, leaving it at the extreme point it has moved to, and, on its return, carries the other along with it, leaving it in the ſame manner at the other extremity. The index and regiſters are carried round the dial-plate very freely, by a weight of 8 grains.

As this inſtrument was intended only to mark the common degrees of heat and cold of this climate, which, according to Fahrenheit's ſcale, is ſeldom above 80, or below 0. I regulated its range by the following proportions, founded on Mr. Smeaton's table of the expanſion of metals, the experiments I had made on ſpelter and braſs correſponding pretty exactly.

Greateſt expanſion of the firſt bar of ſpelter from freezing to boiling water $\frac{353}{10,000}$ parts of an inch per foot, 2 feet long, $\frac{706}{10,000} \times 3$, the power of the firſt lever, $= \frac{2118}{10,000}$.

Ditto of the ſecond bar of hammered braſs, 2 feet 2 inches long, $\frac{487}{10,000} + \frac{2118}{10,000} = \frac{2605}{10,000} \times 3$, the power of the ſecond lever, $= \frac{7815}{10,000}$.

Ditto of the third bar, 2 feet 3 inches long, $\frac{506}{10,000} + \frac{7815}{10,000} = \frac{8321}{10,000} \times 4$, the power of the third lever, $= \frac{33,284}{10,000}$.

Ditto of the fourth bar, 2 feet 6 inches to the place where the threads are hooked on, $\frac{562}{10,000}+\frac{33,284}{10,000}=\frac{33,846}{10,000}$, almoſt $3\frac{4}{10}$ inches, the ſum of the greateſt expanſion of the ſeveral bars, increaſed by the powers of the levers. This is × 30 by the pulley, on the axis of which the index is placed, and carried round a dial 10 inches diameter.

I take ſomewhat leſs than $\frac{1}{3}$ of the greateſt expanſion from freezing to boiling water, to be about a medium of the common degrees of heat and cold of this climate, which makes one revolution of the index. The inward circle, Fig. 2. is divided into 80 parts, correſponding with 80° of Fahrenheit's. Each of theſe is divided into 5 parts on the outward circle, one of which is as large as 2° of Fahrenheit's.

I have compared this inſtrument with a mercurial, and ſpirit thermometer along with it, for ſome time paſt; and have obſerved, that it conſtantly begins to mark the change before either; though the mercury, in ſome time, when the room becomes warm by fire, or otherwiſe, riſes a degree or two above it. When the room is warmed to any great degree, it riſes ſomewhat higher than the mercury, and, at the ſame time, the ſpirit riſes higher than either, though, on the firſt degree of warmth, it does not riſe as faſt as either.

The metalline thermometer has this advantage over any other, that its range may be increaſed to any degree intended. I have one which carries the index 72 inches, by the common changes of the weather, which may be raiſed 50 or 60°, by blowing one's breath five or ſix times on the firſt bar. It marks the 282,000th part of an inch per foot expanſion, and the powers of the levers, are ſo eaſily increaſed, by the help of counterballance weights, that the millionth part of an inch expanſion, or contraction, may be ſhewn; and an inſtrument formed to point out every ſtate of the cold or warmth of the air ſo minutely, as ſcarcely ever to remain ſtationary.

The bars are placed on a board of white deal, ſtraight grained, and free from knots, which was thoroughly well ſeaſoned and dry. I had it varniſhed over ſeveral times with ſtrong varniſh, or japan, to ſecure it from the moiſture of the air, which it ſeems to have done effectually. I have placed it ſeveral times in the open air, when it has rained inceſſantly for many hours, without perceiving any difference in its operation.

I found the regiſters to the thermometer ſo ſatisfactory, and the operation ſo light and eaſy, that I have alſo applied them to the wheel barometer. I had the tube A, Fig. 3. made ſomewhat above $\frac{1}{2}$ inch diameter in the hollow of the tube, with a ball B at the top, above 3 inches diameter, to the middle of which the mercury riſes at a medium. — $\frac{1}{10}$ inch mercury in this part of the ball, is ſufficient to fill 3 inches of the tube; ſo that by making one round of the pulley, on which the index is placed, $\frac{1}{10}$ inch leſs than 3 inches, it makes the riſe and fall of the mercury with more exactneſs, than any barometer, where there is not an allowance made for the ſinking or riſing of the mercury in the ciſtern, the diſtance between the two ſurfaces being the exact height of the mercury. This, I believe, is ſeldom attended to in common barometers; but it requires this exactneſs in a barometer of this kind, as $\frac{1}{10}$ inch riſe or fall in the tube, is increaſed to an inch in the range of a dial-plate 10 inches diameter.

The axis of the index pulley, as alſo the regiſters, are placed on friction wheels, as thoſe of the thermometer; but it requires, that the work be made with greater nicety, in order to lay the leaſt weight on the mercury. I therefore employed Mr. Vulliamy, a watch-maker, and very ingenious mechanic, to make the machinery, which, on trial, has exceeded my expectation, as it requires but the weight of two grains to turn the regiſter and index freely.

The weight *c*, which reſts on the mercury, is made of ivory, in the ſhape of a cone, hollow within, and made narrowing towards the bottom, with a ſcrew in the middle to open; ſo that by pouring in a ſmall quantity of mercury, you may readily adjuſt its weight, which is to be ſo much heavier than the counterballance *d*, as ſerves to turn the index and regiſters. The bottom of the weight *c* is made convex, in order to meet the firſt riſe of the mercury, which is obſerved to ſwell in the middle of the tube, before it can overcome the friction occaſioned by the ſides of the glaſs, and alſo to ſink in that part firſt; by this means, a riſe or fall of 3 or 4 degrees is often obſervable, by the index of this inſtrument, when the mercury in the common barometers ſeems to continue ſtationary.

The weights *c* and *d* are ſuſpended on ſilk threads, as wound off from the cocoons. This kind of ſilk, which is not twiſted, and has the natural gum on it, probably is not in any degree affected by the moiſture or dryneſs of the air. The pulley, on which theſe threads are placed, is made double: that on which the weight *e* is ſuſpended, ſurrounds one part; and the thread on which the counterballance weight *d* is ſuſpended, ſurrounds the other: ſo that when the poſition of the index is properly adjuſted, it cannot eaſily be miſplaced, the weight will always keep in its proper poſition on the ſurface of the mercury, carrying the index and regiſter, as the mercury riſes or falls in the tube.

The inward circle of the dial-plate is divided into three parts, correſponding with 3 inches generally allowed for the riſe and fall of the mercury in common barometers. Each inch is divided into twelve lines, and each line ſubdivided into ten parts, on the outward circle. The regiſters are very ſlender, and mark very diſtinctly half of theſe diviſions, which is the 240th part of an inch riſe of the mercury in the tube.

Many ſudden changes of the temperature of the air, and preſſure of the atmoſphere, have probably paſſed unnoticed, for want of ſome eaſy method of marking the variations with ſufficient preciſion. It has been accidentally remarked, that the mercury has ſunk to a great degree, and roſe very ſuddenly, during the ſhock of an earthquake; but, from the ſuddenneſs of the motion, the degrees could not be aſcertained. Any ſuch ſudden alteration, or even the common changes, will appear with ſo much certainty by the regiſters, that I ſhould imagine, inſtruments of this kind will greatly aſſiſt thoſe, who are obliged to a daily attention, in order to minute the changes that happen with any accuracy; and yet the variations in the night-time, which I have often found greater than in the day, have generally paſſed unnoticed; particularly, in one or two ſtormy nights, I found the index point in the morning near the ſame degree it did, when I placed the regiſters; and yet it appeared, by the regiſter it carried with it, that it had fallen ſeveral degrees during the ſtorm.

I ſhould imagine the metalline thermometer might be employed to ſome uſeful purpoſes, and at no very great expence. For inſtance, a very plain inſtrument of four ſpelter bars, and three levers, might very eaſily be contrived for hot-houſes, which, by a pin fixed in the fourth bar, at a proper place, adjuſted by the botanical thermometer, might be made to raiſe a click, whenever the heat of the houſe raiſed the bar to that point, ſo as to let a ventilator operate by weights, until the air within the houſe became cool to the degree intended, by which the bars would be contracted ſo, as to draw back the click, and ſtop the ventilation; by which means, the houſe might always be kept within any two intended degrees of heat. The weight, which operates the ventilator, might be made to bear on a ſpring, when it comes near the ground, to ring an alarm bell, to warn the attendant to wind up the weight, or awake him for the purpoſe, if aſleep.

A like inſtrument might probably be applied, with great benefit, to rooms where large aſſemblies are collected, and obliged to remain a long time. The unwholſomeneſs of an over-heated air in ſuch places, has been very fully proved, by the late moſt worthy and ingenious Dr. Hales; and yet the danger of ſuddenly throwing in too great a quantity of cold air, when the pores are opened by ſo great a degree of heat, has probably hindered the application of ventilators to this purpoſe. But, by this means, all danger on that account would be avoided with certainty, as the bars could be adjuſted to any two degrees of heat, within which, there could be no danger.

I have ventured thus far on ſpeculation, as I can have no doubt of the power of metals by expanſion; and imagine it will readily be allowed, that a ventilator may be worked by a weight, as well as by wind.

I ſend your Lordſhip a drawing of the barometer and thermometer, and have placed the inſtruments for the inſpection of the gentlemen of the Royal Society, in their meeting-room; where, if agreeable, I ſhall leave them for ſome time.

There have been ſome very ingenious methods contrived, to mark the variations that happen during the abſence of the obſerver; but I do not know, that any attempt has been made in this manner. I wiſh theſe regiſters may be found to anſwer the purpoſe; and am, with great reſpect,

My Lord,

Your Lordſhip's moſt obedient

humble ſervant,

Poland-Street,
May 6, 1761.

Keane Fitzgerald.

XXX. *An Account of the Earthquake felt in the Iſland of* Madeira, March 31, 1761: *By* Thomas Heberden, *M. D. F. R. S. Communicated by* William Heberden, *M. D. F. R. S.*

Read May 21, 1761.

IN the city of Funchal, on the iſland of Madeira, March 31, 1761, we were alarmed with the ſhock of an earthquake, preceded by the uſual noiſe in the atmoſphere, like heavy carriages paſſing haſtily over rough pavements. It began at thirty-five minutes after eleven o'clock in the morning, and laſted (by my watch) full three minutes; the vibrations, which were very quick, remitting and increaſing twice very ſenſibly, during the ſhocks, which ſeemed to be progreſſive, from eaſt to weſt. It has ſeparated ſome rocks in the eaſtern part of the iſland, which have fallen from the cliffs into the ſea. It has likewiſe damaged the walls of ſeveral buildings: among the reſt, my houſe has ſuffered, the ſtone-walls thereof, which are two feet thick, being ſplit in ſeveral places, which has happened in particular to the walls, which ſtand in a direction north and ſouth.

During the earthquake, the fountain of this city (whoſe water is very clear at other times) ran turbid and whitiſh.

The ſea was agitated very ſenſibly, fluctuating ſeveral times between high-water and low-water mark. The fluctuation of the ſea continued longer in the eaſtern parts of the iſland, than in this part.

Though it has been remarked, that a calm always attends an earthquake; no ſuch thing happened now, a fine gale of wind blowing before and after, as well as during the time of, the ſhock.

The ſky was ſerene, interſperſed with flying clouds.

The ſun, which ſhone very bright, immediately after the earthquake was ſurrounded by a very large halo, which laſted about an hour, and gradually diſappeared.

XXXI. *An Account of a Treatiſe in* Latin, *preſented to the Royal Society, intituled,* De admirando frigore artificiali, quo mercurius eſt congelatus, diſſertatio, &c. a J. A. Braunio, Academiæ Scientiarum Membro, &c. *by* William Watſon, *M. D. R. S. S.*

To the Royal Society.

Gentlemen,

Read May 21, 1761.

VERY early laſt year, we were informed, that at Peterſburg, by the means of artificial cold, the mercury in thermometers had been condenſed to ſo great a degree, as to become perfectly fixed and ſolid: but as this information was received only in a looſe way, from the public gazettes, the opinions of philoſophers here were ſuſpended, in relation to their giving credit to this very extraordinary phænomenon, until the truth of it could be ſufficiently authenticated. This has very lately been done by Profeſſor Braun, who firſt made the experiments, and who preſented an account of them to the Royal Academy at Peterſburg, a printed copy of which has been communicated by him to the Royal Society.

Profeſſor Braun obſerves, that every age has its inventions, and that the diſcovery of ſome things ſeem to be reſerved for particular perſons. To this, the hiſtory of ſciences in all ages, more particularly of the late and the preſent, bears witneſs ſufficiently, by the invention of the air-pump, barometers, thermometers, optical inſtruments, electricity, more particularly the natural, artificial magnets, phoſphorus, the diſcovery of the æberration of light, and of many other things in natural philoſophy. He does not know, whether the congelation of mercury, which it was his good fortune to diſcover, may not be ranged among theſe: for who did not conſider quickſilver, as a body, which would preſerve its fluidity in every degree of cold? Neither was the fact otherwiſe, if this is underſtood of natural cold, ſuch as it has been found in any part of the globe, hitherto diſcovered. But if it ſhould happen, that the natural cold ſhould ever be ſo intenſe as artificial cold has been found to be, the whole globe would have a different face, as men, animals, and plants, would certainly be deſtroyed. He did hint ſome time ſince, in a diſſertation upon the degrees of heat, which certain liquors and certain fluids would bear before they boiled, and the degrees of cold they reſpectively bore, before they were converted into ice, that there was a ſuſpicion, that the mercury in ſome of the barometers and thermometers made uſe of for experiments in Siberia had been frozen: but ſince that in greater degrees of cold, the mercury continued fluid in other barometers and thermometers, the immobility and hardneſs obſerved in ſome of theſe inſtruments, was attributed more probably to the lead or the biſmuth, with which the mercury had been adulterated, and was not conſidered as a real freezing of the mercury: but this has been ſince put out of all doubt; ſince it is certain, that pure mercury would not freeze under ſuch ſmall degrees of cold, great as they were for natural cold. The experiments, which the profeſſor made, in order to congeal mercury, demonſtrate this moſt evidently; beſides which, they exhibit new phænomena.

There happened at Peterſburg, on the 14th of December 1759, a very great froſt, equal if not more intenſe than any which had been obſerved there: for, between nine and ten o'clock in the morning, Deliſle's thermometer ſtood at 205; at ſeven o'clock, at 201; which laſt was the greateſt degree of cold, that had been obſerved at Peterſburg, either by himſelf or others. At one o'clock at noon, the thermometer ſtood at 197. Mr. Braun had been employed, ſeveral days before this, in obſerving the ſeveral degrees of cold, which different fluids would bear, before they were converted into ice; partly to confirm thoſe things which he had already laid before the academy; and partly to make experiments upon liquors, which had not yet been examined; as on the days between the 7th and 14th the cold was intenſe enough to be between the degrees of 181 and 191.

When the natural cold was ſo intenſe as to be at 205, Profeſſor Braun conjectured, that it was of all others the moſt proper occaſion to try the effects of artificial cold; not doubting, but that artificial cold would be increaſed in proportion as the natural was more intenſe. Aquafortis, which was found by the thermometer to be 204 degrees cold, was the greateſt part of it frozen, the ice having the appearance of cryſtals of nitre; which, however, immediately diſſolved in a ſmall degree of heat. This aquafortis, which though frozen at the ſides, was liquid in the middle, was poured upon pounded ice, in that proportion which was directed by Fahrenheit, the firſt perſon who made artificial cold with ſpirit of nitre. But before the profeſſor made this experiment, he, by examination, found, that both the ice and aquafortis were of the temperature with the air, which was then 204. Upon the firſt pouring, the mercury fell 20 degrees; this ſpirit was poured off, and freſh put on, ſeveral times; but it was poſſible, by theſe means, to introduce no more than 30 degrees of cold; ſo that the mercury in the thermometer fell no lower than 234. Since therefore Fahrenheit could not produce cold greater than that of 40 below the cypher of his thermometer, which correſponds with 210 of that employed by Profeſſor Braun; nor Reaumur, nor Muſchenbroek, who often repeated the ſame experiment, our author was upon the point of giving up this purſuit; as conſidering this as the greateſt degree to which artificial cold could be carried; thinking it ſufficient honour to himſelf, to have added 20 degrees to the cold formerly known.

But reflecting, that this was not all the fruit he expected from theſe experiments, he determined to purſue them; but at the ſame time, however, to vary the manner of them. By good fortune, his ice was all gone, and he was compelled to uſe ſnow in its ſtead, after having firſt tried, and found the ſnow of the ſame degree of cold with the air, at this time 203. The ſnow, the thermometer, and the aquafortis, being of the ſame temperature, he immerſed the thermometer in ſnow, contained in a glaſs; and, at firſt, only poured a few drops of the aquafortis upon that part of the ſnow, in which the thermometer was immerſed; upon which he obſerved the mercury to ſubſide to 260. Elated by this remarkable ſucceſs, he immediately conceived hopes, that theſe experiments might be carried further: nor was he deceived in his expectations; for repeating the experiment in the ſame ſimple manner, he poured on only ſome more aquafortis, and immediately the mercury fell to 380. Upon which he immerſed the thermometer in another glaſs filled with ſnow, before it had loſt any of this acquired cold; and at length, by this third experiment, the mercury ſubſided to 470 degrees. When he obſerved this enormous degree of cold, he could ſcarce give credit to his eyes, and believed his thermometer broke. But, to his infinite ſatisfaction, upon taking out his thermometer, he found it whole; though the mercury was immoveable, and continued ſo in the open air twelve minutes. He carried his thermometer into a chamber, where the temperature of the air was 125 degrees;

grees; and, after ſome minutes, the mercury being reſtored to its fluidity, began to riſe. But to be certain, whether this thermometer had received any injury, and whether it would yet correſpond with his thermometer, which he keeps as a ſtandard, he ſuſpended them together, and in twenty minutes the thermometers correſponded one with the other.

The thermometers, which our author uſually employs, have a ſpherical bulb, and their ſcale is divided into 1200 parts, of which 600 are above the cypher, which denotes the heat of boiling water, and 600 below that heat. A thermometer of this conſtruction was uſed in inveſtigating the heat of boiling mercury and oils. He had another thermometer, of which the ſcale went no lower than 360 degrees below the cypher, denoting the heat of boiling water. He repeated the former experiment with this, and the mercury very ſoon deſcended ſo, that the whole was contained in the bulb, which, however, it did not quite fill. The mercury in this bulb was immoveable, even though he ſhook the thermometer; until about a quarter of an hour, it began to aſcend in the open air; and it continued to aſcend, till it became higher than the circumambient air ſeemed to indicate. He was ſtruck with this extraordinary phænomenon, and very attentively looked at the mercury in this thermometer, and found certain air bubbles interſperſed with the mercury, which were not in that of the other thermometer. From theſe, and other experiments (it would be unneceſſary to recite them all), he was ſatisfied, that the mercury in theſe thermometers had been fixed and congealed by the cold.

Hitherto our profeſſor had only ſeen the mercury fixed within the bulb of his thermometers. Theſe he was unwilling to break. He was, however, deſirous of examining the mercury in its fixed ſtate, and therefore determined to break his thermometers in the next experiments. It was ſeveral days before he got other thermometers, which exactly correſponded with thoſe he had already employed.

When theſe were procured, the natural cold had ſomewhat relented. In the former experiment, the thermometer ſtood at 204; it was now at 199. In making the experiment, he varied the manner a little. He firſt put the bulb of the thermometer into a glaſs of ſnow, gently preſſed down, before he poured on the aquafortis; he then, in another glaſs, poured the aquafortis upon the ſnow, before he immerſed his thermometer therein; he then, in like manner, put the ſnow to the aquafortis, before he put his thermometer therein. Which ever of theſe ways he proceeded, he found the event exactly the ſame; as the whole depended upon the aquafortis diſſolving the ſnow. When he had proceeded ſo far, as to find the mercury immoveable, he broke the bulb of the thermometer, which had already been cracked in the experiment, but the parts were not ſeparated. He found the mercury ſolid, but not wholly ſo, as the middle part of the ſphere was not yet fixed. The external convex ſurface of the mercury was perfectly ſmooth; but the internal concave one, after the ſmall portion of mercury, which remained fluid, was poured out, appeared rough and uneven, as though compoſed of ſmall globules. He gave the mercury ſeveral ſtrokes with the peſtle of a mortar, which ſtood near him. It had ſolidity enough to bear extenſion with theſe ſtrokes; its hardneſs was like that of lead, though ſomewhat ſofter; and, upon ſtriking, it ſounded like lead. When the mercury was extended by theſe ſtrokes, he cut it eaſily with a penknife. The mercury then becoming ſofter by degrees, in about twelve minutes it recovered its former fluidity, the air being then 197. The colour of the congealed mercury did ſcarce differ from that of the fluid: it looked like the moſt poliſhed ſilver, as well in its convex part, as where it was cut.

The next day, the cold had increaſed to 212 degrees, which was 7 degrees beyond what it had ever before been obſerved at Peterſburg. The ſeaſon ſo much favouring, he thought it right to continue his purſuit, not only in further confirmation of what he had already obſerved, but to inveſtigate new phænomena. In two thermometers, he obſerved the ſame facts in relation to the congealing of mercury, as he did the preceding day. In the bulbs which he broke, the whole of the mercury was not fixed, as a very ſmall portion, much leſs than that of the preceding day, continued fluid. He treated this mercury as he did the former; he beat it with a peſtle, he cut it, and every thing was thus far the ſame. But he ſaw a very great difference in relation to the deſcending of the mercury in the thermometer, the like of which did not occur to him, neither in the former nor any of the ſubſequent experiments. From the former ones it appeared, that the mercury in the firſt experiment had only deſcended to 470, when it became immoveable, though the glaſs bulb was not cracked. In the experiment of the 25th, it deſcended to 530; and in two thermometers on the 26th, to 650. But as well in the thermometer, which he uſed on the 25th, as in two of the 26th, the bulbs were cracked in the experiment: they cohered however; nor was the leaſt part of the bulb ſeparated, but the congealed mercury ſeemed to adhere to all parts of the bulb. In the following experiments, he invariably found, that the mercury ſunk lower, if the whole of it was congealed, than if any part of it remained fluid. It then generally deſcended to 680 and 700, but the bulbs were never without cracks; moreover, it deſcended to 800, and beyond even to 1500; but in this laſt experiment, the bulb was quite broke, ſo that the globe of mercury, thoroughly frozen, fell out, and by its fall, of about 3 feet, the globe of mercury became a little compreſſed; but in the former, only ſome parts of the bulb fell off.

Mr. Braun always found, that, cæteris paribus, the more intenſe the natural cold was, the more eaſy and more expeditiouſly theſe experiments did ſucceed.

In continuing theſe experiments he obſerved, that double aquafortis was more effectual than ſimple ſpirit of nitre; but that if both the aquafortis and Glauber's ſpirit of nitre, which he ſometimes alſo uſed, were well prepared, the difference was not very conſiderable. When his aquafortis was frozen, which often happened, he found the ſame effects from the frozen parts, when thawed, as from that part of it, which remained fluid in the middle of the bottle. Simple ſpirit of nitre, though it ſeldom brought the mercury lower than 300 degrees, by the following method he even froze mercury with it.

He filled ſix glaſſes with ſnow, as uſual, and put the thermometer in one of them, pouring thereupon the ſpirit of nitre. When the mercury would fall no lower in this, he, in the ſame manner, put it in a ſecond, then in a third, and ſo in a fourth; in which fourth immerſion, the mercury was congealed.

Another very conſiderable difference preſented itſelf in purſuing theſe inquiries, with regard to the mode of deſcent of the mercury. He conſtantly and invariably obſerved, that the mercury deſcended at firſt gently, but afterwards very rapidly. But the point, at which this impetus begins, is not eaſy to aſcertain; as in different experiments it begins very differently, and ſometimes at about 300, at other times about 350, and even further. In the experiment before-mentioned, in which the mercury fell to 800, it proceeded very regularly to 600; about which point it began to deſcend, with very great ſwiftneſs, and the bulb of the thermometer was broke. The mercury, however, was perfectly congealed.

He frequently obſerved another remarkable phænomenon; which was, that although the ſpirit of nitre, the ſnow, and the mercury in the thermometer, were previouſly reduced to the ſame temperature, upon pouring the ſpirit of nitre upon the ſnow, the mercury in the thermometer roſe. But as this did not always happen, he carefully attended to every circumſtance; from which it appeared, that this effect aroſe from his pouring the aquafortis immediately upon the bulb of the thermometer, not previouſly well immerſed in the ſnow. He likewiſe obſerved another effect, twice only; and this was, that, after the thermometer had been taken out of the ſnow and aquafortis, the mercury continued to ſubſide, in the open air, down as low as the congealation of mercury.

In the courſe of theſe inquiries, our profeſſor found no difference, whether he made uſe of long or ſhort thermometers; whether the tubes were made of the Bohemian, or the glaſs of Peterſburg. Under the ſame circumſtances, the ſame effects were always produced, making an allowance for the different contraction of the different glaſſes, under ſo ſevere a degree of cold. But if theſe tubes were filled with different mercury, there was then a ſenſible difference; inaſmuch as mercury revived from ſublimate did not ſubſide ſo faſt in the thermometer, as that did, which was leſs pure. He has even found, that he has been able to congeal the leſs pure mercury, at a time when he could not bring the revived mercury lower than 300 degrees: but this he would, till farther trials have been made, not have conſidered as a general axiom.

From theſe experiments, our author conceives it demonſtrated, that heat alone is the cauſe of the fluidity of mercury, as it is that of water and other fluids. If, therefore, any part of the world does exiſt, in which ſo great a degree of cold prevails, as to make mercury ſolid, there is no doubt, but that mercury ought to appear there as a body equally firm and conſiſtent, as the reſt of the metals do here: that mercury, upon congealing, becomes its own ice, however different the mercurial ice may be from that of water, or other liquids. The idea of freezing does or can comprehend nothing more than a tranſition of bodies from a ſtate of fluidity to that of firmneſs by the ſole interpoſition of cold.

The ice of oily and ſaline bodies differs greatly from that of water, which is friable and eaſily broke, whereas that of mercury is ductile. And M. Braun proceeds to conſider all bodies, which liquify by heat, as ſo many ſpecies of ice; ſo that every metal, wax, tallow, and glaſs, comes within his view, in this reſpect.

Mercury then is, in its natural ſtate, a ſolid metal; but is fuſible in a very ſmall degree of heat. Every metal begins to flow in a certain degree of heat; but this degree is different in different metals. Pure tin begins to run at 420; lead, at 530; and biſmuth, at 470, in Fahrenheit's thermometer: or, according to our author, lead liquifies at 320 above the cypher in his ſcale, which correſponds with 596 in Fahrenheit; lead at 170 = 416 of Fahrenheit; biſmuth at 235 = 494; zinc requires a greater heat to melt it than will make mercury boil. Now, if it could be ſettled, at what point mercury would begin to be congealed, we ſhould know the point at which it began to flow; as it has been long known, that water is either fluid or ſolid, as the heat of it is a very few degrees above or under 32 in Fahrenheit's thermometer. Juſt ſo metals become ſolid, at almoſt the ſame degree of heat in which they become fluid. But in mercury, the congealing point is at too great a latitude to be exactly determined; but our author eſtimates it to be about 469 degrees in his thermometer; at a leſs degree than which, he has not been able to obſerve the ſlighteſt congealation. Hence it follows, that the condenſation or contraction, and conſequently the diminution of the volume of mercury muſt be very great indeed. This is demonſtrated by the great deſcent of the mercury in the thermometer, while it is freezing. But how great this diminution of the volume of the mercury is, cannot exactly be determined; and hence ariſes no ſmall difficulty in determining its ſpecific gravity, as this laſt muſt increaſe, as the bulk of the mercury leſſens. Hence as mercury, even in its fluid ſtate, comes of all bodies, platina excepted, the neareſt to gold; in its ſolid ſtate, it muſt ſtill approach much nearer.

Our author had three thermometers filled with the moſt highly rectified ſpirit of wine. Theſe not only correſponded exactly with one another, but, in leſs ſevere trials, correſponded reaſonably well with thoſe filled with mercury. But by the mixture of ſnow and ſpirit of nitre, which froze the mercury, he never was able to bring the ſpirit thermometers lower than 300. From hence it appears, that the heat, which will freeze mercury, will not freeze ſpirit of wine; and that therefore ſpirit thermometers are the moſt fit to determine the degree of coldneſs in frigorific mixtures, until we are in a ſituation to conſtruct ſolid metallic thermometers with ſufficient accuracy.

Our author made many experiments, to try the effects of different fluids, in his frigorific mixtures. He invariably found, that Glauber's ſpirit of nitre and double aquafortis were the moſt powerful. With oil of vitriol, the moſt ponderous of all acids, he was never able to congeal mercury. He likewiſe tried a great number of other fluids, both acid and ſpirituous, which though, when mixed with ſnow, produced cold, it was in very different degrees. He tried a ſeries of experiments to this purpoſe; but it was in weather far leſs cold than the preceding experiments were tried in, viz. between 159 and 153, by his thermometer. By theſe it appears, that ſpirit of ſalt pounded upon ſnow, increaſed the natural cold 30 degrees; ſpirit of ſal ammoniac, 10; oil of vitriol, 35; Glauber's ſpirit of nitre, 58; aquafortis, 40; ſimple ſpirit of nitre, 30; ſpirit of vinegar, and lemon juice, made no remarkable difference; dulcified ſpirit of vitriol, 20; Hoffman's liquor anodynus, 32; ſpirit of hartſhorn, 10; ſpirit of ſulphur, 10; ſpirit of wine rectified, 20; camphorated ſpirit, 15; French brandy, 12; and even ſeveral kinds of wine, increaſed the natural cold to 6, 7, or 8 degrees. That inflammable ſpirits ſhould produce cold, ſeems very extraordinary, as rectified ſpirit ſeems to be liquid fire itſelf; and what ſtill appears more pardoxical is, that inflammable ſpirits poured into water, cauſe heat; upon ſnow, cold: and what is water, but melted ſnow?

Though not immediately relating to the principal purpoſe of this treatiſe, our author meaſured by his thermometer, when it ſtood in his ſtudy at 128 degrees, the heat occaſioned by pouring different fluids into water. He found, that oil of vitriol produced 35 degrees; ſpirit of ſea ſalt, 10; Hoffman's anodyne liquor rectified, 5; ſpirit of wine, 10. On the contrary, ſpirit of ſal ammoniac mixed with ſnow, ſpirit of ſulphur, and ſpirit of hartſhorn, mixed likewiſe with ſnow, made no perceptible difference. Highly rectified chymical oils, mixed with water, produced no heat; nor with ſnow, no cold; as was tried in the oils of turpentine, amber, mint, and mother of thyme.

And here it is to be remarked, notwithſtanding the contrary has been given out by ſome, that theſe chymical oils mixed with the moſt highly rectified ſpirit of wine, do produce no cold, either upon their mixture, or half an hour after.

It reſults from theſe experiments, that although there are many liquids, which can produce artificial cold, the nitrous acid is the moſt powerful; and mercury may be congealed by it, without any difficult proceſs, at any time, when the heat of the atmoſphere is not greater than 175 by the thermometer before-mentioned. And theſe experiments have not only ſucceeded with our author, but with many others; among whom, it may be ſufficient to mention Meſſieurs Lomonoſow, Zeiher, Aepinus, and Model, as theſe gentlemen have made themſelves well known in the philoſophical world. The nitrous acid was poured upon the ſnow, in no determinate quantity; ſometimes a few drops were ſufficient, ſometimes it required a larger quantity. Snow ſeems to be more fit for thoſe experiments, than pounded ice; as the former, from its looſe texture, is of more apt and eaſy ſolution.

Hence it appears, that mercury is no longer to be ranked with the ſemi-metals, but as a perfect one, fuſible, though with a much leſs degree of heat than any of the others. It agrees likewiſe with other metals; as their parts like it, when in fuſion, attract one another, and run into globules, and, from a ſtate of fluidity, paſs into a ſolid ſtate, not all at once, but ſucceſſively, and vice verſâ. But it is worth inquiring, whether this metal, which agrees with all others, both in a ſolid and fluid ſtate, has not the particular property of boiling at a certain degree of heat, which is by no means to be obſerved in other metals. The degree of heat, in which mercury begins to boil, is not at 600 of Fahrenheit's ſcale, as is generally imagined; but at leaſt at 709 of the ſame ſcale, which correſponds with 414 of our author's, whoſe cypher is at the heat of boiling water.

Both the boiling and freezing of mercury have this in common; that when it begins to boil, it riſes with rapidity; and deſcends rapidly, when it begins to freeze. If, therefore, the mean term of the congealation of mercury is fixed at 650 below the cypher, and the term of its boiling at 414 above the cypher; its greateſt contraction to its greateſt dilatation, will be 1064 degrees of our author's thermometer, and 1237 of Fahrenheit's; as 212 is the point of boiling water in this laſt, and 32 the freezing one; which correſponds with 150, under the term of boiling water, in our author's. Hence every one will ſee the great alteration of ſpecific gravity in frozen and boiling mercury, as, between one and the other, the tenth part of the volume is leſſened.

It may be aſked, why the mixture of ſnow and niturous acid does not run into a ſolid maſs, and form itſelf into ice, but remain of a ſoft conſiſtence, although actually much colder, than what is required to freeze aquafortis? We have already mentioned, that aquafortis freezes at 204 of our author's thermometer, which correſponds with 34 below the cypher of Fahrenheit's. The frigorific maſs, in a degree of cold far below this, remained ſoft like a pultice. The cauſe of this extraordinary phænomenon ſeems to be no other than a continuation of the ſolution of the ſnow, and its mixing with the nitrous acid. For as the production of cold depends ſolely upon the ſolution and mixture, it cannot happen, that this maſs, which conſtitutes a fluid of a hard kind, ſhould run into a ſolid conſiſtence, ſo long as the ſolution and mixture continue.

And now, Gentlemen, it requires no ſmall ſhare of your indulgence, to pardon my having extended this account ſo far: but I have to plead in my excuſe, that the ſubject of this work is intirely new, and replete with a vaſt variety of curious facts; all which exactly fall in with our excellent inſtitution. For who, before Mr. Braun's diſcovery, would have ventured to affirm mercury to be a malleable metal? who, that ſo intenſe a degree of cold could be produced by any means? who, that the effects of pouring nitrous acid upon ſnow, ſhould ſo far exceed thoſe, which reſult from mixing it with ice; when ſnow and ice are produced from the ſame ſubſtance, and ſeem to differ only in their configuration? As Mr. Braun's work is in very few hands, I had reaſon to hope, that you would not be diſpleaſed to be informed, in a degree ſomewhat circumſtantial, of theſe very extraordinary facts.

I am,
With the moſt profound reſpect,
Gentlemen,
Your moſt obedient,
humble ſervant,

April 18, 1761.

W. Watſon.

XXXII. *Ob-*

XXXII. *Obſervations on the Tranſit of* Venus *over the Sun, on the* 6*th of* June 1761: *In a Letter to the Right Honourable* George *Earl of* Macclesfield, *Preſident of the Royal Society, from the Reverend* Nathaniel Bliſs, *M. A.* Savilian *Profeſſor of Geometry in the Univerſity of* Oxford, *and F. R. S.*

To the Right Honourable the Earl of Macclesfield, *Preſident of the Royal Society.*

My Lord, June 11, 1761.

Read June 11, 1761.

THE preſent bad ſtate of health of my worthy friend and collegue Dr. Bradley, his Majeſty's Aſtronomer, prevented him from making the proper obſervations of the tranſit of Venus on Saturday morning laſt; and conſequently, has deprived the public of ſuch as would have been taken by ſo experienced and accurate an obſerver. But as the doctor was pleaſed to deſire me to attend at the Royal Obſervatory, to ſupply his place, I have preſumed to lay before your Lordſhip, and the Royal Society, the obſervations I there made, with great care, and as much accuracy, as the unfavourable ſtate of the heavens would permit. The inſtruments we propoſed to uſe, were a reflecting teleſcope, of two feet focal length, to which was fitted Mr. Dollond's micrometer, both executed by Mr. Short. There were ſome additions neceſſary to be made to that inſtrument, which could not be completed before Tueſday evening, the 2d inſtant, and which we received at the Obſervatory, early the following morning. But as it is abſolutely neceſſary, that the teleſcope ſhould be nicely adjuſted to diſtinct viſion, for the obſerver's eye, otherwiſe the apparent angle, meaſured by the micrometer, will not be exactly true; and as the eyes of different obſervers may vary very much; the weather was ſo very unfavourable, that I had not ſo much as one opportunity of ſeeing any celeſtial object, whereby I might fit it to the proper focus of parallel rays for my eye. Mr. Green therefore, Dr. Bradley's aſſiſtant, was the only perſon who could uſe that inſtrument, having adjuſted it to his eye ſome time before. The inſtrument I made uſe of myſelf, was an exquiſite micrometer, of the old form, made by the late Mr. Graham, adapted to an excellent refracting teleſcope of 15 feet focal length. The ſky was ſo very cloudy the morning of the tranſit, and the apparent probability of its clearing up ſo very ſmall, that we almoſt deſpaired of being able to make any obſervation; for we had but one glimple of the ſun, and that only for about half a minute, till half an hour after ſeven o'clock. We then prepared to obſerve the diſtance of Venus from each limb of the ſun, on the chords parallel to the equator, by Mr. Green, with the reflecting teleſcope, and its micrometer; and I myſelf, with the refracting teleſcope, and the old micrometer, obſerved differences of right aſcenſion and declination from the conſequent and ſouthermoſt limb of the ſun.

The weather was more favourable at your Lordſhip's own obſervatory at Shirburn-Caſtle, where the Reverend Mr. Hornſby, Fellow of Corpus-Chriſti College in Oxford, attended, to aſſiſt Mr. Phelps and Mr. Bartlett, your own obſervers. Mr. Hornſby has favoured me with a copy of the obſervations there made; and writes, that though the morning ſeemed very unpromiſing, yet the clouds began to diſperſe about half an hour after five, moving ſlowly towards the eaſt. He then made many obſervations of the differences of Venus and the ſun's limb in right aſcenſion and declination, in the ſame manner which I uſed at the Royal Obſervatory, the ſky free from clouds, and the air tolerably clear. I ſhall not at preſent lay theſe obſervations, or my own, or Mr. Green's, before your Lordſhip and the Society, as the ſhortneſs of the time will not permit me to examine how well they correſpond with each other, or what degree of exactneſs may be depended upon from them.

The continual ſwift motion of flying clouds, of different denſities, over the diſk of the ſun, were no ſmall prejudice to our obſervations at Greenwich, till the end of the tranſit was approaching, when it was tolerably clear, a ſmall hazineſs only remaining. We obſerved the internal contact of Venus with the ſun's limb, Mr. Green having taken off the micrometer with the two feet reflector, Mr. Bird, mathematical inſtrument-maker in the Strand, with a reflector of 18 inches focal length, of his own making, and myſelf with the refractor, the teleſcopes uſed by Mr. Bird and myſelf magnifying about 55 times, that by Mr. Green 120 times, June 5th, 1761, at

20h 19′ 00″ apparent time, all three agreeing to the ſame ſecond. The final egreſs by Mr. Green and myſelf, was only one ſecond later than by Mr. Bird, at 20h 37′ 9″ apparent time. At 20h 26′ 56″, by the mean of five obſervations, the center of Venus was north of the ſun's ſouth limb in declination, by my micrometer 3′ 20″. The diameter of Venus was once meaſured by Mr. Green, with Dollond's micrometer, 57″; by Mr. Canton in Spital-Square, being the mean of three good obſervations, with the ſame kind of micrometer, 58″. The ſun's horizontal diameter was obſerved by Mr. Bird, with the reflector, 31′ 36″, which I ſuſpect is three or four ſeconds too large, as the teleſcope was not accurately adjuſted for parallel rays to his eye.

The internal contact was obſerved by Mr. Hornſby, on the north ſide of the obſervatory at Shirburn-Caſtle, with an excellent 12 feet teleſcope and micrometer, made by Mr. Bird, of the old form; and by Mr. Phelps, on the ſouth ſide, with your Lordſhip's 14 feet teleſcope; the teleſcope uſed by Mr. Hornſby magnifying 68 times, and that by Mr. Phelps about 55 times; by Mr. Hornſby at 20h 15′ 10″ apparent time, by Mr. Phelps four ſeconds later, Mr. Bartlet counting the clock, which each obſerver could hear. Mr. Phelps loſt the final contact, by miſtaking the teller of the clock. Mr. Hornſby makes it at 20h 33′ 17″; but ſuppoſes it to have happened a few ſeconds later; for, at 20h 33′ 12″, it was not quite gone off the ſun, when he was obliged to move his eye-ſtand, and 20″ after, it was certainly totally emerged. They make the diameter of Venus 56″, and Mr. Hornſby, by a mean of twelve obſervations, made a little before and after the noon of the 5th, makes the diameter of the ſun at right angles to the equator, with his micrometer, 31′ 32″. At 20h 12′ apparent time, Mr. Hornſby, by one obſervation, makes the center of Venus north of the ſun's ſouth limb in declination, 3′ 26″. The latitude of the obſervatory at Shirburn-Caſtle is 51° 39′ 22″, being to the north of the Royal Obſervatory 10′ 43″. The difference of longitude between them has been determined, by ſome former obſervations, to be 4′ 1″, that of Shirburn being to the weſt. Theſe are all the obſervations which are come to my knowlege, and which I think, at preſent, worthy the attention of your Lordſhip, and the Royal Society. If the others ſhould hereafter appear to be ſo, they ſhall be laid before you, by

Your Lordſhip's

and their moſt obedient

humble ſervant,

Nathaniel Bliſs.

XXXIII. *An Account of the Tranſit of* Venus *over the Sun, on* Saturday *Morning,* 6th June 1761, *at* Savile-Houſe, *about* 8″ *of Time Weſt of* St. Paul's **, London.

Read June 11, 1761.

HIS Royal Highneſs the Duke of York, being deſirous of obſerving the rare phænomenon of Venus's paſſage over the diſk of the ſun, I had the honour of being commanded by his Royal Highneſs, to attend him on that occaſion at Savile-houſe, which was a place the moſt proper for that obſervation, on account of its remarkable elevation above all the neighbouring buildings, and conſequently above the groſs vapours of the town.

In obedience to theſe orders, on Friday, 5th June, I carried thither the inſtruments proper for this obſervation, together with an aſtronomical clock, made by Mr. Shelton, and the fellow of that which was laſt made for the Royal Obſervatory at Greenwich, and which ſtands in the tranſit-room, and went to the ſaid houſe on Saturday morning, at four o'clock, in company with the Reverend Dr. Blair, and Dr. Bevis, and immediately put the inſtruments in order.

The inſtruments, made uſe of on this occaſion, were a reflecting teleſcope of 18 inches focus, with a helioſcope adapted to it, and having a field of more than the ſun's diameter, proper for ſhewing Venus on the ſun's diſk, with great eaſe and ſatisfaction;

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* N. B. St. Paul's is 22½″ weſt of Greenwich obſervatory.

and another reflector of 2 feet focus, with an achromic object-glaſs micrometer of 40 feet focus, being the ſame ſort of inſtrument with thoſe that were made, by order of the Royal Society, for Dr. Bradley, at the Royal Obſervatory at Greenwich; and for Mr. Maſkelyne, who went to St. Helena; and Mr. Maſon, who went to Bencoolen; differing in one particular from their inſtruments, which had only a common object-glaſs micrometer.

I intended to have meaſured the diſtance of Venus from each limb of the ſun, in chords parallel to the plane of the equator, and in chords parallel to the horizon, and alſo to have taken the appulſes of the limbs of the ſun and Venus to a vertical and horizontal wire, and had all the apparatus neceſſary for thoſe obſervations; but the cloudineſs of the morning prevented my putting any of thoſe methods into practice, for the clouds continued ſo cloſe, that we had no ſight of the ſun, till a quarter of an hour before ſix o'clock, when, through an opening, which laſted for about two minutes, Dr. Blair, Dr. Bevis, and I, plainly and diſtinctly ſaw Venus on the ſun, and concluded, that ſhe was then conſiderably paſt the middle of her tranſit. About a quarter after ſix, I made the firſt obſervation, which was, in meaſuring the diameter of Venus; and ſoon after, I meaſured her diſtance from the ſun's limb, in the direction of a line going through the ſun's center; and ſo continued meaſuring in the ſame manner, and ſometimes meaſuring the diameter of Venus, till near the internal contact; only about a quarter after ſeven, I meaſured the diſtance of Venus from the ſun's limb, in a ſuppoſed direction of her tranſit line, or path over the ſun.

About half an hour after ſeven, the clouds diſperſed, and we had the ſun perfectly clear during the remainder of the tranſit. When Venus approached the internal contact, I took off the micrometer, and changed the magnifying power of the teleſcope, which, during the meaſurements, had been that of 70 times, into another of 140 times, and with this magnifying power, I obſerved the internal contact; in which, I think, I cannot have erred ſo much as two ſeconds, for the air was extremely clear, and at reſt. With the ſame magnifying power, I obſerved the total exit; and I do not think I have erred in this above five ſeconds, though this is a more uncertain obſervation than the former, and can by no means be determined ſo accurately as the internal contact; and what I have erred in this laſt obſervation, is rather in exceſs, in making the exit too late.

I have mentioned, above, the magnifying power of the teleſcope I uſed; becauſe I have found, by experience, that the different lengths of teleſcopes, their different magnifying powers, and their different goodneſs, as well as the different goodneſs of eyes, want of practice, and different ſtate of the air, will produce differences of times in thoſe ſort of obſervations.

Theſe obſervations were made in the preſence of his Royal Highneſs the Duke of York, accompanied by their Royal Highneſſes Prince William, Prince Henry, and Prince Frederick; her Royal Highneſs Lady Auguſta was pleaſed likewiſe to do us the honour of looking at this uncommon appearance.

Times

Times and meaſurements, taken at Savile-Houſe, on Saturday morning, 6th June 1761. Mr. Short obſerving, and Dr. Bevis marking down the times.

h	'	"		'	"	
5	46	37	firſt ſaw Venus on the ſun.			
6	15	12	diameter of Venus - =	0	59.8	
6	20	44½	neareſt diſtance of the limbs	4	48.2	of the ſun and Venus.
6	31	05	ditto - - - - - - - -	4	28.5	
6	50	24	ditto - - - - - - - -	3	49.9	
6	52	38	ditto - - - - - - - -	3	44.8	
6	54	23	ditto - - - - - - - -	3	44.0	
6	59	37½	diameter of Venus - - =	1	00.7	

Theſe preceding obſervations were taken in the intervals of clouds, and ſomewhat in a hurry.

h	'	"		'	"	
7	01	42½	diameter of Venus - =	0	58.9	
7	05	36½	neareſt diſtance of the limbs	3	20.1	
7	08	05½	ditto - - - - - - - -	3	12.4	
7	09	28½	ditto - - - - - - - -	3	08.5	
7	11	27½	ditto - - - - - - - -	3	05.5	
7	12	53	ditto - - - - - - - -	3	01.3	
7	18	22	ditto - - - - - - - -	2	48.4	
7	19	54½	diſtance of the limbs in a	4	03.6	ſuppoſed line of Venus's path.
7	22	03½	neareſt diſtance of the limbs	2	37.3	
7	24	23½	ditto - - - - - - - -	2	32.2	
7	26	09½	ditto - - - - - - - -	2	27.0	
7	31	58½	ditto - - - - - - - -	2	13.7	
7	37	24½	ditto - - - - - - - -	1	58.9	
7	40	59½	ditto - - - - - - - -	1	49.5	
7	41	30½	diameter of Venus - =	0	58.9	
7	43	20	ditto - - - - - - - =	0	58.9	
7	44	26	neareſt diſtance of the limbs	1	39.2	
7	47	30	ditto - - - - - - - -	1	30.2	
7	52	01	ditto - - - - - - - -	1	16.9	
7	55	41	ditto - - - - - - - -	1	08.4	
8	01	08	ditto - - - - - - - -	0	52.1	
8	04	32½	ditto - - - - - - - -	0	42.7	
8	18	21½	Internal contact by Mr. Short, through a reflector of 2 feet focus, magnifying 140 times.			
8	36	12½	Total exit by Dr. Blair, through a reflector of 18 inches focus, magnifying 35 times.			
8	37	05½	Total exit by Mr. Short, through a reflector of 2 feet focus, magnifying 140 times.			

The diameter of the ſun, in a horizontal direction, was meaſured juſt after the tranſit, and found to be = 31′ 30.8″.

The clock at Savile-Houſe was ſeveral times compared with my clock in Surry-Street, from Friday evening, the 5th June, to Monday evening, the 8th June; ſo that I am as ſure of the time at Savile-Houſe, as if the obſervation had been made at my houſe in Surry-Street.

Ja. Short.

XXXIV. *Obſervations on the Tranſit of* Venus, June *the 6th*, 1761, *made in* Spital-Square; *the Longitude of which is* 4′ 11″ *Weſt of the Royal Obſervatory at* Greenwich, *and the Latitude* 51° 31′ 15″ *North*; *by* John Canton, *M. A. and F. R. S.*

Read Nov. 5, 1761.

HAving meaſured the diameter of Venus, on the ſun, three times, with the object-glaſs micrometer, the mean was found to be 58 ſeconds; and but $\frac{6}{10}$ of a ſecond, the difference of the extremes *.

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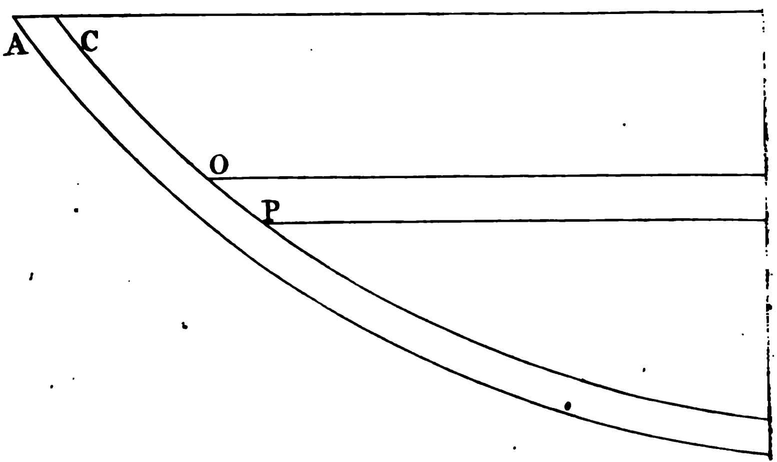
* With the ſame micrometer, the diameter of Venus was meaſured, off the ſun, twelve times, March the 29th, 1758, about noon; and the mean was 1′ 1″ 42‴; whence the diameter, at the time of the tranſit, ought, by computation, to have been 1′ 0″ 19‴.

The


A
C
O
P

The diameter of the ſun, from four obſervations very nearly agreeing with each other, was 31′ 33″ 24‴.

	h.	m.	ſ.
The time, by the clock, of the internal contact, was - - - - - - -	8	17	4
Of the external contact -	8	35	27
Of noon - - - - -	11	58	24½
Therefore the apparent time of the firſt contact, was - - - - - - -	8	18	41
Of the laſt contact -	8	37	4

The two poſitions of Venus on the ſun's diſc, [*Vide Tab.* VI.] in chords parallel to the equator, were determined by frequently meaſuring the parts of ſuch chords on each ſide the centre of the planet, with the object-glaſs micrometer; which was done with difficulty, both on account of the clouds, and the teleſcope's not having an equatorial motion.

Let the arc A T B repreſent a part of the ſun's limb; let C F D be parallel to it, at the diſtance of a ſemidiameter of Venus; and let O V X and P V Y be parallel to the equator. At $7^{h}\ 14^{m}\ 39^{\text{ſ}}$ A. M. apparent time, O V was 14′ 43″, and V X 5′ 32″. At $7^{h}\ 57^{m}\ 21^{\text{ſ}}$, P V was 16′ 36″, and V Y 1′ 56″.

Theſe obſervations were all made with a reflecting teleſcope of 18 inches focal length, which magnified about 55 times.

XXXV. *Some Obſervations of the Planet* Venus, *on the Diſk of the Sun,* June *6th,* 1761; *with a preceding Account of the Method taken for verifying the Time of that Phænomenon*; *and certain Reaſons for an Atmoſphere about* Venus: *By* Samuel Dunn.

Read Nov. 5, 1761.

AS ſoon as I heard, that ſeveral mathematicians were to go abroad, and obſerve the tranſit of Venus over the diſk of the ſun, from foreign parts, I purpoſed to obſerve the ſame at Chelſea, and to compare my obſervation with the more accurate ones, which I expected would be made at the Royal Obſervatory at Greenwich. Suppoſing, that if my obſervation at Chelſea was made with equal care, and as good inſtruments for aſcertaining time, as the inſtruments to be uſed by the obſervers abroad, it might be uſeful, as a kind of proof, how exact the obſervations had been made abroad, where the obſervers had leſs conveniencies for aſcertaining time, than at the Royal Obſervatory.

Mr. Dollond (a member of the Society) had, ſome time before, newly ground, and fitted up for me, a Newtonian reflecting teleſcope, ſix feet in length; which ſo far exceeded expectation, that by it the Reverend Mr. Maſkelyne (who is now at St. Helena) and myſelf, had ſeveral times obſerved how long Jupiter's ſatellites dwell on the limb of Jupiter, entering on the body. And I was provided with an eight-day clock, having a ſecond-hand, an iron pendulum, and an adjuſting ſcrew at its bob.

Altitude inſtruments of wood and braſs, adjuſted by ſpirit levels, had engaged my attention; but having found ſeveral of theſe defective, by comparing their reſults with meridian altitudes, and the time by the clock; and having many times examined the clock by double altitudes of the ſun, taken with a Hadley's quadrant, having a nonius to minutes, and an artificial horizon of ſweet oil in a tea-ſaucer, I determined to depend on ſuch an inſtrument and horizon, for aſcertaining the error of the clock, and correctneſs of my meridian.

An artificial horizon of water, and even of quickſilver, I had found to be too eaſily diſturbed, and therefore had, ſome time before, introduced oil, and found it vaſtly preferable. And in taking altitudes, I always obſerve, when the ſun, or other celeſtial body, is as near the prime vertical, or eaſt and weſt azimuth, as poſſible; and generally take either five double altitudes, half a minute of time aſunder each, or three double altitudes, a minute of time aſunder, dividing the ſum by either ten or ſix, as the caſe is, for a mean ſingle altitude, correſponding to the mean time of thoſe obſervations by the clock. And in taking the ſun's tranſit acroſs the meridian, I take a mean of the times of appulſe to ſeveral parallel and equidiſtant lines on each ſide of the meridian, and it generally gives the tranſit to leſs than a ſecond of time.

The daily tables of the ſun's declination, equation of time, &c. which I uſe, are thoſe in the ephemerides of the Abbé de la Caille; and the latitude of my place is 51° 29′ 5″ N. and 41″ of time weſt of the obſervatory at Greenwich, between the phyſic-garden and Chelſea hoſpital.

Several obſervations * for verifying the quadrant, meridian, clock, &c. made at different times of the year, were as follow; viz.

22d December 1760. Altitude ſun's upper limb on the meridian 15° 6′ 30″. Error by calculation 2″ of a degree.

26th December 1760. Altitude ſun's upper limb on the meridian 15° 12′ 15″. Error by calculation 8″ of a degree.

2d January 1761. Altitude ſun's upper limb on the meridian 15° 20′ 42″. Error by calculation 3″ of a degree.

18th February 1761. Altitude ſun's upper limb on the meridian 27° 22′ 45″. Error by calculation 4″ of a degree.

6th February 1761. Sun on the meridian, by the clock, at 12^{h} 14′ 35″. Error by equation table 0″ of time.

11th February 1761, morning, at 10^{h} 20, per clock. Altitude ſun's upper limb 20° 23′. Error of clock 0″ of time.

11th February 1761, noon, ſun on meridian, per clock, at 12^{h} 14′ 44″. Error by equation table 0″ of time.

11th February 1761, afternoon, at 6^{h} 37′ 30″ per clock. Altitude ſun's upper limb 6° 37′ 30″. Error of clock 0″ of time.

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* A great number of others were made, although not inſerted in this paper.

From 18th February to 11th March, the clock had gained of equal time 37″, which is near 2″ of time per day.

11th March to 10th April, the clock had gained of equal time 15″, which is near 3″ of time per day.

4th June, afternoon, at 4ʰ 52′ per clock. Altitude ſun's centre 27° 40′ 14″, which is near 3″ of time per day.

6th June, afternoon, at 5ʰ 40′ per clock. Altitude ſun's centre 24° 29′ 5″, which is near 3″ of time per day.

8th June, afternoon, at 5ʰ 20′ per clock. Altitude ſun's centre 23° 45′ 53″, which is near 3″ of time per day.

9th June, noon, ſun on the meridian, per clock, at 12ʰ 59′ 32″, which is near 3″ of time per day.

Theſe latter obſervations, ſhewing the gain of the clock 3″ of time per day, ſurprized me, as being contrary to my expectation; for the clock had loſt two or three ſeconds per day in winter, and therefore, I concluded it would loſe more in the ſpring and ſummer, by the lengthening of the pendulum; but it happened quite the contrary, and the cauſe thereof I could not determine.

For aſcertaining the diameter of Venus, and alſo the poſition and diſtance of the ſolar maculæ from Venus, I had cauſed to be conſtructed an inſtrument *, much like one which has been already deſcribed to the Society,

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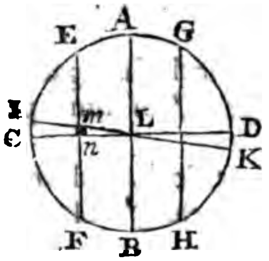
* Angular micrometer.

ciety, with this addition, which was originally deſigned for that inſtrument; namely, E F and G H, two ſilver wires parallel to the diameter A B, and diſtant therefrom 30° = A E = A G = B F = B H, the three parallel wires E F, A B, and G H, being fixed, whilſt the moveable wires C D and I K opened to any angle, as I L C for intercepting the diameter *m n* of Venus, whilſt C D was perpendicular to A B, and the planet was divided into two equal parts by the wire E F. This micrometer was placed in the eye-piece of a two feet Gregorian teleſcope, which magnified 55 times, and through the field of view, of which the ſun paſſed in 118 ſeconds of time.



I had two eye-glaſſes to the ſix feet Newtonian * reflector; one of which, being ſix tenths of an inch focus, magnified 110 times; and the other, being three tenths of an inch focus, magnified 220 times, or four times that of the Gregorian reflector. The greater of theſe two glaſſes I purpoſed to truſt to, having often experienced its ſuperiority in viewing the occultations of Jupiter's ſatellites and the ſolar maculæ.

The idea I had formed of the internal contact was, that the planet would touch the edge of the ſun in an inſtant, like two drops of quickſilver meeting on a plane, and that in an inſtant the black contact would appear; but in this I was deceived, the particulars of the phænomenon being as follows; viz.

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* The diameter of the great ſpeculum of this teleſcope was ſix inches.

June 6th, a cloudy morning, till about ſix o'clock, when the clouds began to diſſipate, but not enough to afford a plain ſight of Venus on the ſun, till more than half paſt ſeven, and the planet got nearer the limb of the ſun, than I had deſired to ſee it firſt on the diſk.

By repeated trials, the time in which the planet was paſſing from the internal to the external contact, with the wire of the micrometer was $3\frac{1}{4}$ ſeconds of time, and the angle ICL of the angular micrometer was 8°.

With the ſix feet Newtonian reflector, and its magnifying power of 110, and alſo of 220 times, I carefully examined [*] the ſun's diſk, to diſcover a ſatellite of Venus, but ſaw none; for I had a very clear dark glaſs next my eye, and the ſun's limb appeared moſt perfectly defined; but a very narrow wateriſh penumbra [†] appeared round Venus, by which its limb was not perfectly defined, and at the diſtance of about a ſixth part of Venus's diameter from its edge, was the darkeſt part of Venus's phaſis, from which to the centre, an imperfect [‡] light increaſed, and illuminated about the centre.

At 8^h 16′ per clock, I was prepared to obſerve the internal contact; and as Venus drew nearer to the limb of the ſun, the penumbra near the limb of Venus became darker, and threatened to obſcure the point of contact at the inſtant it would happen; the circumſtances of which, for each of the moments of time, are imperfectly delineated, on account of the nearneſs of the lines, but more truly deſcribed as follows; (a right line repreſenting that part of the ſun's limb near where the contact happened, and an arch the approaching limb of Venus, for each three ſeconds of time, from the loſs of the thread of light.)

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[*] After the tranſit, till two o'clock afternoon the ſame day, I continued obſerving the diſk with this teleſcope, but ſaw no ſatellite paſs over the ſun.

[†] This penumbra could not by any means be made to diſappear, although I tried to make it vaniſh, by altering the focus of the teleſcope a great number of times.

[‡] This could not ariſe from any imperfection of the teleſcope, as the ſolar maculæ appeared ſharply defined, as through a refractor.

A diagram, repreſenting the approach of Venus to the ſun's limb, for each three ſeconds of time. [*Vide Tab.* VII.]

In this diagram, the black ſegments repreſent Venus, and the right lines drawn nearly at contact to them, repreſent a ſmall part of the ſun's limb, as ſeen through a dark glaſs; the intermediate ſpaces white, repreſent the ſky. In words, (for each ſecond of time by the clock) thus:

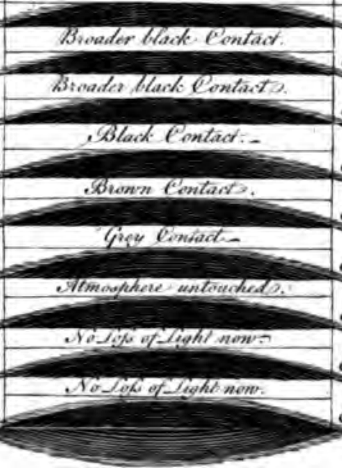
	h	′	″	
At	8	16	41	No diminution of light between the limb of Venus and that of the ſun.
	8	16	42	Slight penumbra, or diminution of light, near where the contact was to be.
	8	16	43	Penumbra of a grey colour, near the ſame place.
	8	16	44	Penumbra almoſt brown, and the thread of light very narrow, and almoſt loſt.
	8	16	45	Penumbra brown, and the thread of light in the contact point indiſtinct, or loſt.
	8	16	46	Penumbra more brown, and the touch the ſmalleſt poſſible.

At

VENUS with an Atmosphere, as seen on the Face of the SUN 6th June 1761, thro' a Telescope magnifying 220 times in Diameter. Also the Approaches of the Planet toward the Sun's Limb, & the Circumstances referred to other Telescopes & to the Royal Observatory at Greenwich. By Samuel Dunn.

A Table deduced from this Observ.n shewing the apparent Time of the Contact by different Tel.s at Greenwich & at Paris. p. S.D.

Power	Greenwich Observatory	Paris Observatory
	h ′ ″	h ′ ″
150	8.18.21	8.28.16
100	8.18.53	8.28.18
75	8.18.55	8.28.20
60	8.18.57	8.28.22
55	8.18.58	8.28.23
50	8.18.59	8.28.24



Approaches deduced for a Tel. of 80 times for Paris. Paris Observatory.	Approaches deduced for a Tel. of 80 times for Greenwich. Greenwich.	Approaches of the Planet toward the Sun's Limb, at the Internal Contact.	Time p. Clock with a Tel. of 220 Power at Chelsea.	Apparent Time with a Tel. of 220 Chelsea.	Apparent Time for Greenwich, by a Telescope of 220 by this Observation.	Apparent Time for Paris, by a Telescope of 220 Power by this Observation.
h ′ ″ 8.28.35	h ′ ″ 8.19.10	Broader black Contact.	h 16.58 ″	h 8.18.14 ″	h ′ ″ 8.18.55	h ′ ″ 8.28.20
8.28.27	8.19. 2	Broader black Contact.	8.16.55	8.18.11	8.18.52	8.28.17
8.28.19	8.18.54	Black Contact.	8.16.52	8.18. 8	8.18.49	8.28.14
8.28.11	8.18.46	Brown Contact.	8.16.49	8.18. 5	8.18.46	8.28.11
8.28. 3	8.18.38	Grey Contact.	8.16.46	8.18. 2	8.18.43	8.28. 8
8.27.55	8.18.30	Atmosphere untouched.	8.16.43	8.17.59	8.18.40	8.28. 5
8.27.47	8.18.22	No Loss of Light now.	8.16.40	8.17.56	8.18.37	8.28. 2
	8.18.14	No Loss of Light now.	8.16.37	8.17		
			8.16			

Philos Trans. Vol LII. TAB. VII. p. 190.

	h	′	″		
At	8	16	47	Penumbra almoſt black, and the touch a little broader.	
	8	16	48	Slight black in the point of contact, and the edges a little broader.	
	8	16	49	True black in the point of contact, and the edges a little broader.	
	8	16	50	More ſo.	Here I concluded with myſelf, that obſervers would differ in their judgments about the moment of contact, ſome ſeconds of time, or that ſome would eſtimate the contact ſooner than others.
	8	16	51	More ſo.	
	8	16	52	More ſo.	

From theſe obſervations, I concluded, that the thread of light in the point of contact was ſo obſcured, as to be undiſcernible at $8^h\ 16'\ 46''$; and that true black did not ſucceed in the ſame point, till $3''$ after, namely, $8^h\ 16'\ 49''$; and from * both of theſe properties,

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* As the ſix feet Newtonian teleſcope magnified four times as much as that of the two feet Gregorian teleſcope, and the vaniſhing of the thread of light, from its leaſt degree of duſkiſhneſs to a true black, was about 3 ſeconds of time by the ſix feet teleſcope, the time in which the thread of light was vaniſhing from the leaſt degree of duſkiſhneſs to a true black, by a two feet Gregorian reflector, may be ſuppoſed to have been 4 times 3 = 12 ſeconds of time; and hence an error, or rather difference, of pronunciation, but not of judgment, may have ariſen among good obſervers, if ſome eſtimated the contact by the inviſibility of the thread of light, and others by an apparent blackneſs in the point of contact, or, which is the ſame thing, the time when the planet had made the leaſt apparent dent in the ſun's limb, of

perties, I concluded, that the real internal contact was at $8^h\ 16'\ 47''$ by the clock; which makes $8^h\ 16'\ 11''$ equal time, and $8^h\ 18'\ 2''$ apparent time, at Chelſea; and $8^h\ 18'\ 43''$ apparent time, at Greenwich.

Whilſt Venus was on the ſun's limb, no other penumbra appeared between the limb of Venus and the ſun, than had appeared before on the ſun's diſk; and therefore, I concluded there muſt be an atmoſphere about Venus, which, receiving weak impreſſions of light between the limbs of Venus and the ſun, occaſioned the uncertainty of aſcertaining the exact inſtant of the internal contact, as above deſcribed; and becauſe my Newtonian reflector ſhewed objects clearer than the generality of Gregorian reflectors, I concluded, that the aforegoing property was what no two feet reflector was capable of examining, the atmoſphere being ſo narrow.

At $8^h\ 35'$ per clock, the external contact was near, and not incumbered with ſuch a penumbra, or partial light, as the internal contact had been. At $8^h\ 35'\ 4''$, the leaſt dent poſſible, quite black, appeared in the ſun's limb. And at $8^h\ 35'\ 6''$, the limb was reſtored to its perfect form, there having been a ſmall trembling light, between the narrow watery border of Venus and the vaniſhing point of contact in the ſun's limb for theſe two ſeconds of time. From which the

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of the ſame colour, through a dark glaſs, as the ſky. This was verified by a two feet Gregorian reflector, in the contact abovementioned, and poſſibly may have occaſioned greater differences in eſtimating the contact, with leſſer teleſcopes, to no leſs than half a minute of time.

external contact at Chelſea was 8^h 34′ 30″ equal time, and 8^h 36′ 21″ apparent time; which makes 8^h 37′ 2″ apparent time at Greenwich.

From the aforegoing circumſtances, it appeared to me, that the external contact was more eaſily to be determined than the internal one, which was contrary to what I had before expected; and becauſe the point of contact muſt have appeared through ſuch a teleſcope as I obſerved with, in its proper colour, dark or black, ſooner than through a ſmaller magnifying power of equal light, I concluded, that, through my teleſcope, the internal contact was viſible, ſooner than through a two foot reflector, ten or twelve ſeconds of time.

And, conſidering the aforeſaid penumbra, or border of partial light, ſurrounding Venus, as an atmoſphere of that planet, with the time of its vaniſhing, $2\frac{1}{5}$ ſeconds of time; and reducing this to the diameter of Venus, with due allowance for the oblique direction over the ſun's limb, the atmoſphere of Venus comes out $8\frac{1}{2}$ thirds of a degree, which is nearly about $\frac{1}{400}$th part of Venus's diameter; which diameter being nearly equal to the earth's diameter, the atmoſphere of Venus comes out nearly 50 geographical miles.

As theſe obſervations were made with care and attention, I have lain them before the Society only; and the more readily, as they reconcile a ſeeming contradiction in Mr. Short's * numbers of the internal contact;

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* It having been 3 ſeconds of time from the inſtant when the thread of light between Venus and the ſun became ſo indiſtinct, as not to be properly termed light, to the inſtant when the black contact

contact; and, whilſt I am very certain with reſpect to the particulars of the external contact, cannot determine why they differ from that ingenious obſerver's numbers, or any other's.

Samuel Dunn.

⁂ When the limb of Venus was almoſt clear of the ſun's diſk, I perceived a difficulty would occur at the laſt contact, as the limb of the ſun, and alſo that of Venus, which was to make the

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contact appeared through a teleſcope magnifying 220 times, the limits of time in which it continued in a like apparent ſtate, by a teleſcope magnifying $\left\{\begin{matrix}140\\55\end{matrix}\right\}$ times, may be ſuppoſed to have been $\left\{\begin{matrix}5\\12\end{matrix}\right\}$ ſeconds of time; and as $\left\{\begin{matrix}\text{Savile-Houſe is } 30''\\\text{Chelſea is - } 41''\end{matrix}\right\}$ weſt of Greenwich, the black contact, by the above obſervation, with $\left.\begin{matrix}\text{Mr. Short's teleſcope at Savile-Houſe}\\\text{the teleſcope at Greenwich obſervatory}\end{matrix}\right\}$ was at $\left\{\begin{matrix}8^h\ 18'\ 18''\\8\ \ 18\ \ 58\end{matrix}\right\}$ the obſervations at thoſe places being $\left\{\begin{matrix}8^h\ 18'\ 21\frac{1}{2}''\\8\ \ 19\ \ \ 0\end{matrix}\right\}$ And, for other teleſcopes, the limits above-mentioned may be ſuppoſed to have been nearly as in the following table.

Magnifying power.	Limits.
200 times.	3″ of time.
100	7
80	8
60	11
50	13
40	16
35	19
30	22
25	26
20	33
10	66

laſt dent, approached ſo near to a right line. This led me to conſider, that the ſpherical external angle of contact, by the ſix feet teleſcope, would be but an * eighth part of the ſame angle by the two feet teleſcope, the eye judging of the ſame relative diſtance in one teleſcope as in the other; and that, therefore, as the verſed ſine of the dent in the ſun's limb was but a fourth part as large in one teleſcope as the other, and the laſt contact vaniſhed in about 2½ ſeconds of time, the laſt contact might poſſibly be eſtimated, by a two feet Gregorian teleſcope, about twice 2½, or 5″ or 6″, ſeconds of time later than with a ſix feet Newtonian teleſcope; which allowance being made, the apparent time of the two contacts, as reduced to a Gregorian reflector, magnifying 55 times: by ſuch a teleſcope, the contacts at Greenwich obſervatory were, viz. internal contact $8^{h}\ 18'\ 55''$, external contact $8^{h}\ 37'\ 7''$. The accounts which have been publiſhed of the obſervations made at Greenwich being, internal contact $8^{h}\ 19'\ 0''$, external contact $8^{h}\ 37'\ 9''$; and the difference in each within five ſeconds of time, an error anſwering to about a 500th part of the ſun's diſtance from the earth.

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* As the ſpherical angle of contact in the limb of Venus, and alſo of the ſun, was four times as great in one teleſcope as in the other, the ſum of both is eight times; which, being diminiſhed by four times the apparent magnitude of the verſed ſine of the leaſt viſible dent in the one teleſcope, what it was in the other teleſcope, leaves the one double to that of the other.

XXXVI. *An Account of the Obſervations made on the Tranſit of* Venus, June 6, 1761, *in the Iſland of* St. Helena: *In a Letter to the Right Honourable* George *Earl of* Macclesfield, *Preſident of the Royal Society, from the Rev.* Nevil Maſkelyne, *M. A. and F. R. S.*

My Lord,

Read Nov. 5, 1761.

I Am ſorry I cannot have the honour of gratifying your Lordſhip, and the Royal Society, with an account of a more complete obſervation of the tranſit of Venus, than what I herewith tranſmit to you. From the very cloudy weather, which prevailed here for the whole month preceding the tranſit, I, indeed, almoſt deſpaired of obtaining any ſight of it at all. I was, however, fortunate enough to obtain two fair views, though but of ſhort continuance, of this curious celeſtial phænomenon. The firſt was a few minutes after ſun-riſe, when I was ſurprized not only at ſeeing Venus ſo very large, but alſo ſo much nearer the ſun's limb, than I had reaſon to expect from the beſt grounded calculations; which laſt circumſtance foreboded, that ſhe would make a more ſpeedy exit from the ſun's body, than the ſame calculations allowed; which accordingly happened. At this time, her limb, as well as the ſun's, appeared exceedingly ill defined, which was no more than what one might naturally expect, from their great proximity to the horizon.

This alone was ſufficient to prevent my making any obſervations, at that time, which could admit of any exactneſs, if the clouds had not preſently come up, and totally deprived me of the ſight both of Venus and the ſun. In this manner the ſkies continued unfavourable for about an hour, when they grew again extremely clear, and I had the pleaſure of ſeeing Venus appear as an intenſely black ſpot upon the ſun's body, and perfectly well defined. At this time, I meaſured the diſtance of the neareſt limbs of Venus and the ſun from each other, with the curious object-glaſs micrometer adapted to the reflecting teleſcope, according to Mr. Dollond's ingenious invention. This diſtance was $1'\ 44\frac{3}{4}''$, at $7^{h}\ 31^{m}\ 7^{\text{ſ}}$ apparent time, or $7^{h}\ 29^{m}\ 15^{\text{ſ}}$ mean time. I think it proper to take notice, that though Venus's limb and the ſun's appeared as well defined, as could be deſired; yet, when the artificial internal contact of Venus's limb with the ſun's was made, in order to meaſure their diſtance, Venus's limb alternately dilated itſelf over, and contracted itſelf within the ſun's limb, by a ſmall ſpace. I endeavoured to take it in the middle of this vibration; but I beg leave to refer it to your Lordſhip's opinion, whether, if the real internal contact had happened at this time, it could have been obſerved, in ſuch circumſtances, to that degree of exactneſs, which the great Dr. Halley hoped for; and whether, on occaſion of the next tranſit, which is to happen eight years hence, it might not be convenient, that the obſervers ſhould endeavour to place themſelves on ſuch parts of the globe, as that they may not ſee Venus on the ſun's body, very near the horizon, but rather when they are both elevated to conſiderable heights; which will afford them a greater chance of making their obſervations free from clouds, which uſually ſkirt the horizon, as well as of making them to advantage.

Preſently after I had meaſured the diſtance of Venus from the ſun's limb, the clouds returned again, and prevented me, not only from making any more obſervations of the ſame kind, or meaſuring Venus's diameter, but alſo, what was of much more conſequence, from obſerving the laſt internal contact of Venus from the ſun's limb, which was the principal obſervation of all. About 23 minutes after eight, the clouds ſeparated again, and the ſun appeared very bright and clear; but there was not the leaſt appearance to be ſeen of Venus, though I thought myſelf in a manner ſure of obſerving at leaſt the external contact, as all the calculations make the end to happen much later.

Mr. Waddington took the paſſages of Venus and the ſun's limbs, acroſs the horizontal and vertical wire of the equal altitude inſtrument. All the obſervations, which he was able to make, are as follow:

June 6,
Mean time, in the morning.

H.	M.	S.	
7	24	1	☉'s lower limb at horizontal wire.
7	24	18	♀'s centre at vertical wire.
7	27	43½	♀'s centre at the horizontal wire.
	28	50½	♀'s preceding limb touches vertical wire.
	29	9½	☉'s ſubſequent limb at vertical wire.

H.

H.	M.	S.	
7	31	56	☉'s lower limb at horizontal wire.
	32	17	♀'s centre at the ſame.
	33	5	☉'s weſtern and ſubſequent limb at vertical wire.
	34	53	☉'s upper limb at horizontal wire. The obſervation of this limb of the ſun was but indifferent.

N. B. As the teleſcope inverts, the obſervations, as uſual, are ſet down according to the appearance.

I heartily wiſh the other attenders upon this rare celeſtial phænomenon may have had a more favourable opportunity of making their obſervations, than I have had. But as it is to be feared, that our other obſervers, Mr. Maſon and Mr. Dixon, by the misfortunes they have met with, have not been able to make their obſervations at Bencoolen, as was propoſed; I humbly hazard my opinion, and ſubmit it to your Lordſhip's better judgment, whether the difference in the total duration of the tranſit of Venus over the ſun's diſk, obſerved in any two places, where, it is likely, obſervations have been made, will be great enough to enable us to infer the ſun's parallax with ſufficient exactneſs, or even nearer than it is known already. So that I am afraid we muſt wait till the next tranſit, in 1769, which is, on many accounts, better circumſtanced than this, before aſtronomers will be able to do juſtice to Dr. Halley's noble propoſal, and to ſettle, with the laſt and greateſt degree of exactneſs, that curious and nice element in aſtronomy, the ſun's parallax, and thence determine the true diſtance of all the planets from the ſun, and from each other.

Your Lordſhip will excuſe me, that I do not at preſent attempt to deduce any conſequences from the above obſervations, not only as I am in want of others correſponding to them made in other places, but alſo as I am not yet able to ſettle the longitude of this place to ſufficient exactneſs; though I am of opinion, it cannot differ much from that, which Dr. Halley hath aſſigned to it. I have not yet been able to get one obſervation of an eclipſe of Jupiter's ſatellites, though I have not failed, on my part, of being ready to ſeize any opportunity, if it had offered; the very cloudy weather, which prevails at this time, which is the winter here, depriving me not only of theſe, but alſo almoſt all other obſervations.

I cannot conclude, my Lord, without making one remark, that if the late noble Dr. Halley were now alive, he could not receive greater pleaſure from ſeeing the obſervation of the tranſit of Venus undertaken by aſtronomers of different nations, conformably to his propoſal, than from finding it ſo warmly eſpouſed by your Lordſhip, and the Royal Society, to whom, as a perpetual body, whoſe care it would be always to watch over the intereſt and advancement of ſcience, he particularly recommended it. Nor can the learned world but look upon themſelves as highly indebted to your Lordſhip, for that noble zeal, which you have manifeſted for the improvement of aſtronomy, in ſetting forward, and promoting, theſe literary expeditions, which tend to the benefit of mankind, and the honour of our native country.

Taking things in this light, we may preſume to ſay, that our deſign was not unworthy the attention of his late Excellent Majeſty, our Patron, who ſo nobly ſupported us in defraying the expences of theſe expeditions; for whoſe memory we are bound always to retain the higheſt reſpect.

Unfavourable circumſtances may, perhaps, have prevented us from reaping all the benefit, that might be hoped for from theſe undertakings; but they can never deprive us of the ſatisfaction of thinking, that we have done all, that lay in our power, to anſwer and fulfil ſuch noble and important views. I have the honour to be,

My Lord,

With the greateſt reſpect,

Your Lordſhip's

moſt obedient,

and devoted

humble ſervant,

Nevil Maſkelyne.

XXXVII. *An Account of the ſame Tranſit; by the Reverend Mr.* Richard Haydon: *In a Letter to* John Bevis, *M. D.*

To the Reverend Thomas Birch, *D. D. Secretary to the Royal Society.*

Dear Sir,

Read Nov. 12, 1761. I Send you incloſed, the Reverend Mr. Haydon's obſervation of the late tranſit of Venus; and ſhould have waited on you with it long ere now, as I promiſed, but that I unluckily got a fall, which ſtill confines me at home. I preſume this obſervation may deſerve the notice of the Society, as the beſt circumſtanced of any I have yet ſeen made in England: for ſeveral of the phaſes are earlier than thoſe at Greenwich, or ours at Savile-Houſe, taken with a teleſcope of Mr. Short's, armed with Mr. Dollond's new micrometer, and his time accurately aſcertained.

Dear Sir,
Your moſt obedient
and affectionate
humble ſervant,
J. Bevis.

Clerkenwell-Cloſe,
Nov. 12, 1761.

By many compariſons of different obſervations, I make Mr. Haydon's latitude to be 50° 26′ 55″, and his longitude weſt of London in time, 16 minutes 10 ſeconds nearly; though he, from a memorandum he made ſome years ago, ſuppoſed it near two minutes more.

To John Bevis, *Doctor of Phyſic.*

S I R, Leſkeard, June 9, 1761.

I Should, with great pleaſure, have purſued, in every particular, the method you recommended to me, in obſerving the late tranſit of Venus, but, unfortunately, had it not in my power to do ſo. The low ſituation of my houſe, and a ſmall hill at a diſtance to the N. E. of it, would not allow me, even from my garret windows, a view of the ſun, till it was 11° or 12° above the horizon. By this means, I was deprived of an opportunity of making two of the principal obſervations. It was almoſt half an hour after five, when I could firſt get a ſight of the ſun. I was in hopes, from what you had wrote me, that the planet had not at that time paſſed its neareſt diſtance from the centre; but had ſoon the vexation to find myſelf diſappointed. I, however, continued to obſerve the diſtance of Venus from the limb of the ſun, with as much accuracy as I could, an account of which you have on the next leaf. I think there cannot be an error of more than two or three ſeconds in the time of the interior contact, and not one of the total egreſs.

It was but two days before I received the favour of your letter, that I came down ſtairs for the firſt time, after a ſix weeks ſevere fit of the gout. During my illneſs, my clock was run down, and ſtopt. I immediately ſet it going again, as nearly to the time as I could then gueſs. The next day, being the firſt of this month, at night I obſerved the tranſit of a ſtar over the horizontal hair in the teleſcope of my quadrant. The third day, I repeated the ſame, and again laſt night: by which you will ſee my clock meaſures time correctly enough. Thurſday, the 4th, was very hot and ſultry all day; the evening, hazy and foggy. Fearing the night following might prove the ſame (as it unluckily did), and that I ſhould not be able to take the equal altitudes of any of the ſtars before and after they had paſſed the meridian, I obſerved, on Friday, ſeveral correſpondent altitudes of the upper limb of the ſun, in the morning and afternoon; by which the time may be preciſely enough aſcertained. At the bottom, I trouble you with an account of theſe obſervations, as alſo of ſome made the day following.

It gives me much concern, that I cannot herein anſwer your expectations, in a more perfect and ſatiſfactory manner; but, I aſſure you, Sir, I did every thing in my power for that purpoſe, and ſhould moſt readily embrace any opportunity of teſtifying the reſpect I owe you. I beg you'll preſent my compliments to Mr. Short; and am,

S I R, &c.

R. Haydon.

Correſpondent altitudes of the upper limb of the ſun, June the 5th, 1761.

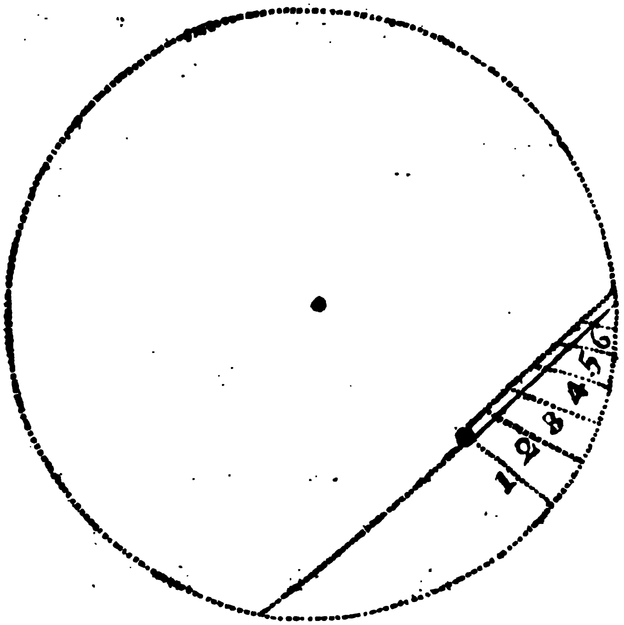
Altitude.		A. M.			P. M.		
°	′	h	′	″	h	′	″
24	7	At 6	51	42	5	27	34
25	0	6	57	9	5	22	7
31	0	7	35	10	4	44	8
32	0	7	41	25	4	37	51
41	56	8	44	53	Cloudy.		
42	40	8	49	35			

By the above, my clock too faſt in apparent time 9′ 34″.

June 6th, upper limb of the ſun.

Altitude.		A. M.			P. M.		
°	′	h	′	″	h	′	″
41	24	At 8	40	57	3	38	38
42	40	8	49	13	3	30	13
44	10	8	59	0	3	20	20

By theſe, clock too faſt 9′ 40″.



June 6th, 1761.

		Inch.	Ten. Pts.		Angular value. ′	″
Diameter of ☉	=	3	20.3	=	31	31.5
Diameter of ♀	=	0	10.0	=	0	59.0

Neareſt diſtance of Venus from the limb of the ſun.
N. B. The diameter of Venus is included.

Obſ.	By my clock.				Micromet.			Angular value.		
		h	′	″				′	″	
1.	At	5	34	54	45	1	23	5	51.6	Semidiameter of Venus to be deducted for the path of the center.
2.		5	53	0	45	1	12	5	38.7	

Obſ.

Obſ.	By my clock.			Micromet.			Angular value.		
	h	'	"				'	"	
3.	At 6	13	2	45	5	17	5	15.1	Semidiameter of Venus to be deducted for the path of the center.
4.	6	31	24	45	0	13	4	40.8	
5.	6	54	54	0	4	22	4	22.0	
6.	7	28	19	0	3	3	3	0.5	
	8	10	0	Internal contact.					
	8	29	3	Total egreſs.					

Star croſſed the hair of my teleſcope, &c.

	Altitude.												
	°	'	h	'	"		h	'	"		h	'	"
June 1.	25	40	10	35	6	June 3.	10	27	13	June 8.	Cloudy.		
	25	0	10	39	10		10	31	26		Cloudy.		
	24	20	10	43	24		10	35	36		10	15	53

N. B. Mr. Haydon informs me, in a ſubſequent letter, that "on comparing his obſervations "with thoſe made in London, his interval be-"tween the internal contact and total egreſs, "was conſiderably longer than any of the "others. Wherefore, he examined his notes "again, but could not find he had made any "miſtake in tranſcribing them." He adds, that "being obliged to obſerve from an upper "window, his regulator being fixed below, "but within hearing, he got a lad, of about "fourteen, whom he ſtrictly charged to be "particularly attentive to the ſecond ſhewn by

" the clock, whenever he ſhould call to him;
" in which reſpect, he is of opinion, he made
" no miſtake, though, poſſibly, he might make
" one with regard to the minute, by ſetting down
" *one too many* at the egreſs; which he now
" thinks there is ſome cauſe to believe he did."

J. B.

XXXVIII. *Obſervations on the ſame Tranſit; and on an Eclipſe of the Moon,* May 8, 1761; *and of the Sun, on the 3d of* June 1761: *In a Letter to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society, from Mr.* Peter Wargentin, *Secretary to the Royal Academy of Sciences in* Sweden, *and F. R. S.*

Read Nov. 12, 1761.

Litteræ hiſce incluſæ, ad te et clariſſimum Dollondium ſcriptæ, quas amicus meus, Dominus Klingenſtierna (principis Suec. hæreditarii præceptor) tranſmittendas mihi tradidit, occaſionem mihi ſuppeditant, paucis te inviſendi, tibique communicandi obſervationes nonnullas aſtronomicas, nuper à me habitas, in obſervatorio Stockholmienſi, cujus elevatio poli eſt 59° 20′ 31″, differentia autem meridian. ab obſervatorio Grenovicenſi 1^h 12′ 1″.

Si illas dignas judicaveris, quæ illuſtriſſimæ Societati Regiæ offerantur, erit id mihi gratiſſimum. Quid-

quid ſit, me tua in ſcientias merita magni facere profiteor, et ſincero cum affectu ſum,

Reverendi nominis tui
Cultor ſtudioſiſſimus,

Petr. Wargentin,
Acad. R. Scient. Suec. Secret.

Stockholmiæ,
die 9 Junii 1761.

Eclipſeos Lunæ totalis, die 18 *Maii hujus anni, obſervatæ, quædam momenta.*

	h	′	″	
PEnumbra denſa in margine Lunæ percipitur - - - - - - -	9	31	30	veſp.
Initium veræ eclipſeos circiter æſtimatum - - - - - - - -	9	32	20	
Grimaldus totus immergit - - -	9	35	50	
Schickardus totus abſconditur - -	9	38	48	
Galilæus occultatus in umbra diſparet	9	39	47	
Gaſſendus deliteſcit - - - - -	9	43	28	
Ariſtarchus umbram ingreditur -	9	48	10	
Tycho incipit immergere - - -	9	51	5	
——— totus abit - - - - -	9	52	18	
Copernicus incipit tenebris offundi	9	54	16	
——— totus fere abſconditus -	9	55	48	
Eratoſthenes immergit - - - -	10	4	0	
Umbra ad Platonem - - - -	10	14	53	
Archimedes evaneſcit - - - -	10	15	26	
Plato totus eſt in umbra - - -	10	16	50	
Plinius ſe ſubducit - - - - - -	10	18	31	
Proclus hæret in margine umbræ -	10	29	38	
Vix apparet veſtigium Procli - -	10	30	28	
Mare Criſium incipit immergere -	10	31	30	
Idem totum tenebræ occuparunt -	10	35	24	
Immerſio Lunæ totalis in umbram	10	41		vel paulo ſerius.

Margo tamen Lunæ, qui ultimus immerſerat, ſatis clarâ luce conſpicuus fuit per 5 vel 6 temporis minuta, et referebat nudis oculis ſpeciem ſtellæ ſecundi ordinis. Hora autem 10^h 52′ ille ipſe margo, cum tota reliqua Luna, ita prorſus diſparuerat, ut nullum ejus veſtigium, vel nudis vel armatis oculis, ſenſibile reſtaret, cœlo licet ſereno, et ſtellis vicinis in tubo conſpicuis. Sub ipſa quoque immerſione, illa Lunæ portio, quæ in umbram inciderat, penitus evaneſcebat, aliter ſane quam fieri ſolet etiam in eclipſibus Lunæ centralibus; plerumque enim Luna, quamvis in medio umbræ, apparere ſolet lumine quodam ſubobſcuro, per atmoſphæram refracto.

Antequam autem Luna ſic diſpareſceret, animadverti ſtellam (Libræ) in vicinia marginis orientalis, quam Luna mox occultatura videbatur.

	h	′	″
Eam omni ſtudio proſecutus vidi demum, oculi ictu citius, exſtingui occultatam	10	52	39
Ejuſdem ſtellæ emerſio contigit inter 11^h 43′ 8″ et - - - - - - -	11	43	28

Emergebat à parte Lunæ prorſus inviſibili: ejuſque ſub Luna ſemita borealior paulo erat diametro Lunæ horizontali.

Poſtquam Lunam amiſſam diu quæſiviſſem, reperi tandem tubo bipedali, hora 11^h 30′, vix ſenſibili luce circa marginem orientalem ſuffuſam. Hora 11^h 33′ ejus quoque veſtigium acutioribus ſe offerebat oculis, inſtar tenuiſſimæ nubeculæ. Pluſquam dimidius autem Lunæ diſcus occidentalis manſit prorſus inviſibilis, et erant limites inter viſibilem et inviſibilem partem valde tortuoſi; circa margines autem Lunæ erat lumen illud intenſius et magis extenſum, quam circa centrum. Cœpta vero emerſione, quidquid nondum emerſerat, plane viſum non feriebat.

	h	′	″
Lunæ margo juxta Grimaldum lucidior exſplendeſcere cœpit	12	13	0
Initium emerſionis veræ circiter	12	15	0
Grimaldus incipit emergere	12	18	37
———— totus luci reſtitutus	12	19	32
Galilæus prominet	12	20	37
Ariſtarchus apparet	12	22	2
———— jam totus eſt illuſtris	12	22	47
Copernici nucleus lucem adſpicit	12	37	46
Tychonis nucleus incipit eluctari tenebris	12	44	35
———————totus evaſit	12	45	52
Plinius prorepit	13	0	47
Promontorium acutum incipit prominere	13	6	50
Mare Nectaris totum emerſit	13	9	37
Snellius emergit	13	12	31
Totum Mare Criſium luci reſtitutum	13	17	26
Langrenus porrigit latus	13	18	15
Finis vera eclipſeos circiter	13	21	8
Non niſi penumbra in Luna eſt reſidua	13	23	0

Obſervationes hæ habitæ ſunt cum tubo 9 pedum. Dominus Strömer, aſtron. prof. Upſalienſis, ſociam mihi in obſervanda hac eclipſi commodavit operam, uſus tubo 5 pedum. Ille pleraque immerſionis momenta 20″ vel 30″ citius, emerſionis autem tantundem fere tardius notavit.

Eclipſis Solis, die 5 Junii.

	h	′	″
Sol ortus eſt hac die quartâ fere diametri horizontali parte mulctatus, hora	3	0	0 mane.
Finis tantum hujus eclipſeos utcunque obſervari potuit; contigit ille	3	12	32

Tranſitus Veneris per diſcum Solis, die 6 Junii.

	h	′	″	
Venus jam aliqua ſui parte diſcum Solis occupaverat. - - - - -	3	21	37	A. M.
Propter vehementem marginum Solis undulationem, primum contactum exteriorem accuratius notare non potui.				
Contactus interior, vel immerſio totalis, meo quidem judicio contigit - - - - - - - - -	3	39	23	
At D^us^ Klingenſtierna contendit, eum demum contigiſſe - -	3	39	29	
Initium emerſionis, contactu interiore, certè mihi apparuit - -	9	30	8	
Idem contactus ex obſervatione Domini Klingenſtierna - - -	9	30	11	
Finis emerſionis vel contactus ultimus, ex judicio D^i^ Klingenſtierna	9	48	6, vel 8.	
Meo autem, neutiquam ante - -	9	48	9	

Ego hac occaſione adhibui tubum 20 ped. Suec. cum oculari, focum ad 3 digit. diſtant. habente.

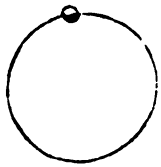
Dominus autem Klingenſtierna uſus eſt novo illo excellentiſſimo tubo Dollondiano 10 pedum, cum oculari medio, vel mediocriter objecta ampliante.

Reliquas meas obſervationes circa hunc tranſitum nondum in juſtum ordinem redegi.

Diameter Veneris in Sole erat quamproxime 5″.

Notatu dignum eſt, quod margo Veneris, qui jam emerſerat, conſpiciendum ſe præbuerit etiam extra

Solem, debili quadam luce, idque ſub tota fere emerſione, in hunc fere modum.



Sive illa in margine Veneris apparens, inflectioni radiorum Solis, ſive refractioni in atmoſphæra Veneris, ſit tribuenda, diſquirant alii.

XXXIX. *An Account of the Obſervations made on the ſame Tranſit in* Sweden: *In a Letter from Mr.* Peter Wargentin, *Secretary to the Royal Academy of Sciences in* Sweden, *and F. R. S. to Mr.* John Ellicot, *F. R. S. Tranſlated from the* French.

SIR, Stockholm, Aug. 7, 1761.

Read Nov. 12, 1761. IN a letter, dated June 8th, I communicated to Dr. Birch, Secretary to the Royal Society, my obſervations upon the tranſit of Venus. Having ſince received ſome other good obſervations of this phænomenon, I thought the communicating them to you, would give you ſome pleaſure.

At Torneo in Lapland, Meſſieurs Lagerborn and Hellant very happily obſerved both the entrance and exit of Venus, with teleſcopes of 32 and 20 feet focal lengths. The principal times obſerved were as follows:

Exterior

	Exterior contact at the entrance.	Interior contact at the entrance.	Interior contact at the exit.	Total exit.
	h ′ ″	h ′ ″	h ′ ″	h ′ ″
Lagerborn	3 45 44	4 4 1	9 54 22	10 12 18
Hellant	3 45 51	4 3 59	9 54 8	10 12 22

Mr. Hellant is eſteemed a very good obſerver. The difference between the meridians of Paris and Torneo, is computed to be $1^h\ 27'\ 28'$, very nearly.

At Abo, the capital of Finland, ſituated in latitude 60° 27′, longitude eaſt of Paris $1^h\ 19'\ 17''$, Mr. Juſtander obſerved with a teleſcope of 20 feet;

	h	′	″
The interior contact, at the entrance to be at	3	55	50
Beginning of the exit, at - - - - - -	9	46	59
Total emerſion, at - - - - - - - -	10	4	42

At Hernoſand, a city in Sweden, in latitude 60° 38′, and longitude $1^h\ 2'\ 12''$ eaſt of the meridian of Paris, Meſſieurs Giſter and Strom obſerved, with teleſcopes of 20 feet.

	h ′ ″	h ′ ″	h ′ ″	h ′ ″
Mr. Giſter	- - - -	3 38 26	9 29 21	9 46 40
Mr. Strom	3 20 40	3 38 35	- - - -	9 46 47

At the obſervatory at Upſal, Meſſieurs Strömer, Metlander Mallet, and Bergman, made the following obſervations, with three teleſcopes of 20 feet, and a reflector of 18 inches. The difference of meridians between Upſal and Paris is $1^h\ 1'\ 10''$.

	h	′	″	h	′	″	h	′	″	h	′	″
Mr. Mallet	3	20	45	3	37	56	9	28	3	9	46	29
Mr. Strömer	- - - -			3	38	5	9	28	7	9	46	13
Mr. Bergman	- - - -			3	37	43	9	28	9	9	46	30

At Lund in Scanie, Mr. Schenmark obſerved, with a teleſcope of 21 feet, The interior contact of the exit was $9^h\ 10'\ 44''$, doubtful, being cloudy; total emerſion $9^h\ 29'\ 14''$. This city is $43'\ 50''$ to the eaſt of the meridian of Paris.

According to the obeſervations made at the obſervatory at Stockholm, by Mr. Klingenſtierna and myſelf;

	h	′	″	h	′	″	h	′	″	h	′	″
Mr. Klingenſtierna	- - - -			3	39	29	9	30	11	9	48	8
Mine	3	21	37	3	39	23	9	30	8	9	48	9

The difference of meridians between Paris and Stockhom is $1^h\ 2'\ 50''$ or $52''$, at moſt.

In theſe obſervations, I made uſe of an excellent teleſcope, of 21 Swediſh feet, and Mr. Klingenſtierna obſerved with one of Mr. Dollond's teleſcopes, of 10 feet, with an eye-glaſs fitted to it, which magnified the object more than 140 times.

In comparing theſe obſervations together, you will perceive, that they do not agree ſo near as was hoped for; and thoſe which were made at Paris, agree but little better. I ſhould be very glad to hear how the Engliſh obſervers have ſucceeded, both at London, and at St. Helena; I therefore deſire you would procure for me thoſe obſervations, which have been communicated to the Royal Society, to whom you may

preſent theſe from Sweden, if you think them deſerving your notice, which I hope you will.

I have the honour to be,

With the moſt perfect eſteem,

S I R,

Your moſt obedient

humble ſervant,

Wargentin.

XL. *Obſervationes* Veneris *ſub* Sole *viſæ, habitæ* Pariſiis, *die* 6ᵃ Junii 1761, *in palatio* Luxemburgi, *quas Regiæ Societati* Londinenſi, *venerationis ſuæ obſequium, offert* Hieronymus De la Lande, *Acad. Reg. Scient.* Pariſinæ *Socius, Regiuſque Matheſeos Profeſſor.*

Read Nov. 19, 1761.

NUbes in oriente perſiſtentes ab horâ quartâ uſque ad ſeptimam viſum Solis rapuerunt, earumque intervalla unicam obſervationem permiſerunt, quæ ſequitur, cum ſextante hexapedali factam.

Temp.

Temp. app.			
h	′	″	
6	31	6½	Limbus præcedens ☉ ad filum verticale.
6	31	43	Limbus præcedens ♀ ad filum horizontale.
6	31	49½	Limbus præcedens ♀ ad verticale.
6	32	42½	Limbus ſequens ☉ ad horizontale.

Ex hâc obſervatione concluditur longitudo Veneris à parallaxi liberata 2′ 35″ ad occaſum centri Solis, latitudo Veneris 9′ 58″.

Deinde adhibui micrometrum objectivum ex duplici vitro ad 18 pedes focum extendentibus, quocum diſtantias limbi utriuſque Veneris à limbo Solis remotiori dimenſus ſum repetitis obſervationibus.

Temp. app.			Diſtantia.		
h	′	″	′	″	
7	13	55	28	11.1	Limbus Veneris auſtralis.
7	18	55	27	23.3	Limbus boreus.
7	21	45	28	32.1	A.
7	25	5	27	39.8	B.
7	27	50	28	42.0	A.
7	30	46	27	49.5	B.
7	37	1	28	7.3	B.
7	38	22	28	12.0	B.
7	39	40	28	15.6	B.
7	41	44	29	19.0	A.
7	44	46	29	24.1	A.
7	48	13	29	33.3	Limbus auſtralis.
7	51	48	28	46.6	Limbus boreus.
7	53	35½	29	53.4	Limbus auſtralis.

Appropinquante exitu, ut latitudo puncti Solis, in quo Venus limbum deſereret, exactius determinaretur, ad ſextantem meum hexapedalem reverſus ſum, et ſequentia obſervavi.

h	′	″	
7	57	21	Limbus præcedens Solis ad verticale.
7	57	34	Limbus præcedens Veneris ad verticale.
7	57	57½	Limbus præcedens Veneris ad horizontale.
7	59	16	Limbus ſequens Solis ad horizontale.

Ex his differentia longitudinis 8′ 21″, latitudo 10′ 39″, horâ 7h 57′ 40″.

h	′	″	
8	1	37	Limbus præcedens Solis ad verticale.
8	1	43½	Limbus Veneris præcedens ad horizontale.
8	1	54½	Limbus ſequens Veneris ad verticale.
8	3	3	Limbus ſequens Solis ad horizontale.

Unde differentia longitudinum 8′ 36″, latitudo 10′ 40″, ad 8h 1′ 50″.

h	′	″	
8	4	47½	Limbus præcedens Solis ad verticale.
8	4	50½	Limbus ſequens Veneris ad horizontale.
8	4	58½	Limbus præcedens Veneris ad verticale.
8	6	5½	Limbus ſequens Solis ad horizontale.

Itaque ad 8h 4′ 54″, differentia longitudinum 8′ 48″, latitudo Veneris 10′ 34″.

h	′	″	
8	13	1	Limbus præcedens Veneris ad horizontale.
8	13	8½	Limbus præcedens Solis ad verticale.

h	'	''	
8	13	16	Limbus præcedens Veneris ad verticale.
8	14	22½	Limbus ſequens Solis ad horizontale.

Ex his ad 8h 13′ 8″, differentia longitudinum 9′ 21″, latitudo 10′ 52″.

His adjungere liceat, in confirmationem, quatuor determinationes habitas prope Lutetiam cum micrometro ad tubum hexapedalem compoſito, ab ill. Abbate De la Caille.

Temp. app. Lutet.			Diff. longit.		Latitudo.	
h	'	''	'	''	°	'
7	49	40	7	48.4	10	44½
7	54	27	8	3.3	10	49¾
7	58	38	8	16.3	10	53
8	13	3	9	20.2	10	59½

Omnes diſtantiæ ac determinationes præcedentes ab effectu refractionis et parallaxeos (10.2″) immunes ſunt, non vero ab effectu aberrationis.

Tandem obſervationibus aliquantulum ſepoſitis, ut viſus acies conquieſceret, cœlo ſereno, tubo decem et octo pedum cum oculari 2½ pollicum, aperturâ 1 pollicis, utroque planetâ exacte circumſcriptis, omnibus apprime diſpoſitis atque faventibus, exactiſſime contactum obſervavi ad 8h 28′ 25″, aut 26″ ad ſummum; ultimum vero ad 8h 46′ 54″, temp. appar. ſeu vero.

Quod ad primum contactum attinet, nullum dubium mihi videtur, quin ad 2″ ubique obſervari poſſit, ut celeberrimus Halley primus monuit ad ſummam aſtronomiæ utilitatem; poſtremus vero contactus 4″ incertitudinem forte ſuſcipiet, niſi tubis longiſſimis obſervatus fuerit.

Ex duratione exitus diametrum Veneris colligere licet 57″.8 diametrum Solis liceat ſupponere 31′ 33″, ut exactiſſimis obſervationibus æſtate præcedente compertus ſum conſentientibus (ut nuntiatum eſt) celeberrimi aſtronomorum nunc facile principis Bradleii obſervationibus; parallaxim Solis 10″.2 adhibeo, unde correctis diſtantiis ſuprà relatis, et cum diſtantiâ Veneris exeuntis comparatis, decem modis conjunctionem Veneris ſcrutatus ſum.

Itaque, medio aſſumpto, ſtatuendum mihi videtur tempus app. conjunct. 5^h 52′, cum latitudine 9′ 30″, longitudine nodi exiſtente 8^s 14° 32′ 20″; atque ita proxime nodum jam inveneram, collatis obſervationibus Pariſinis cum motu nodi Veneris ab actione terræ et Jovis oriundo, quem 20″.37 annuatim, calculo inſtituto, determinavi: has autem determinationes à Societatis Regiæ theſauro primitus oriundas, atque ipſius tranſitus Veneris utilitates brevi futuras ejuſdem Societatis quaſi beneficium, agnoſcamus.

XLI. *An*

XLI. *An Account of the Obſervations on the ſame Tranſit made in and near* Paris: *In a Letter from Mr.* Benedict Ferner, *Profeſſor of Aſtronomy at* Upſal, *and F. R. S. to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society. Tranſlated from the* French.

Reverend Sir,

Read Nov. 19, 1761.

TWO reaſons engage me to pay you my reſpects, on occaſion of the paſſage of Venus over the ſun's diſk, on the 6th paſt: the firſt is my duty to give you ſome account of what was done here upon this article; and the other is founded upon the intereſt I have, in being informed how Venus was obſerved with you.

Meſſieurs Maraldi, De la Lande, and De Liſle, with Mr. Meſſier, remained in town, at the Royal Obſervatory, in the palace of Luxembourg, and at the Hotel de Clugny: Meſſieurs De la Caille, Le Monnier, De Fouchy, and myſelf, went out to Conflans, St. Hubert, and to the Chateau de la Muette, where the King's philoſophical and optical chamber is. It was in this laſt place, which is ſituated $14\frac{1}{2}''$ of time to the weſt of the Royal Obſervatory, that I made my obſervations, in company with Monſ. De Fouchy.

In order to take the diſtances of Venus from the limbs of the ſun, for want of a good micrometer, I made uſe of a quadrant of $2\frac{1}{2}$ feet, made by Langlois; and for obſerving the egreſs, I had a good reflecting teleſcope of 28 inches focus and 5 inches aperture, which magnified about 80 times: the teleſcope was made by Pere Noel. I will not enlarge upon the precautions I took for the benefit of my obſervations, in order to aſſume a vain pretenſion of having attained to the laſt preciſion; it is ſufficient to aſſure you, I made them in the beſt manner, that the circumſtances would admit of. Having calculated and reduced my obſervations to the Royal Obſervatory at Paris, I found, that the weſtern limb of Venus touched the weſtern limb of the ſun, or, that the luminous thread of the ſun was broke by her,

	h	′	″	
At - - - - - - -	8	28	29	True time, morn.
Laſt contact, at - -	8	46	43	
Conjunction of ☉ with ♀	5	52	20	
Southern latitude of -	0	9	32	
	°	′	″	
Longitude of ☋ being 2ˢ	14	32	23	

I ſometimes meaſured the diameter of Venus with a bad micrometer; but finding there was but little account to be made of it, I diſcontinued the uſe of it.

During the obſervation, I had recourſe to different coloured glaſſes; to wit, a black glaſs, ſuch as is made at glaſs-houſes; a common ſmoked glaſs, and a glaſs of a blue and green mixed, half of which was ſlightly ſmoked. In uſing the black glaſs, the diſk of the ſun, and that of Venus, were badly defined, and the ſpots of the ſun appeared but faintly. I ſaw a little better with the common ſmoked glaſs, and with the half-ſmoked green and blue glaſs: but when I viewed

I viewed the ſun with that part of theſe glaſſes, which was not ſmoked, I found the ſun white, and a little bluiſh, the ſmalleſt ſpots very diſtinct, and the diſks of the ſun and Venus much better defined, than I could with the other glaſſes; but the edges of both limbs undulating, particularly thoſe of Venus.

During the whole time of my obſerving with the teleſcope, and the blue and green glaſſes, I perceived a light round about Venus, which followed her like a luminous atmoſphere, more or leſs lively, according as the air was more or leſs clear; its extent altered in the ſame manner; nor was it well terminated, throwing out, as it were, ſome feeble rays on all ſides. When I looked through the ſmoked part, I ſaw but badly; by the common ſmoked glaſs, yet worſe; and by the black glaſs, not at all.

The interior contact of Venus with the ſun's limb happened ſooner than I expected, by judging of the diſtances of Venus from the limb of the ſun at different times. As for the laſt contact, I ſhould not be ſurprized, if there was a difference of 10″ or 12″ between two obſervers, who had inſtruments and eyes of equal goodneſs, and made their obſervations by the ſame clock; ſo difficult a matter do I think it to determine the exact moment. But for the firſt, I certainly believe, that the difference could ſcarce amount to more than 2″ under the ſame circumſtances.

However, the following are greater differences for the firſt contact, than were expected:

		h	′	″
Mr. Maraldi obſerved	1ſt contact at	8	28	42
	2d contact at	8	46	54

L'Abbé

		h	′	″	
L'Abbé de la Caille	1ſt contact at	8	28	37	
	2d contact at	8	46	49½	
Mr. Meſſier obſerved	1ſt contact at	8	28	27	
	2d contact at	8	46	37	
Pere Noel - - -	1ſt contact at	8	28	27	
Mr. Fouchy - -	2d contact at	8	46	42	
Mr. Ferner - -	1ſt contact at	8	28	29	
	2d contact at	8	46	43	
Mr. de la Lande -	1ſt contact at	8	28	25	to 26
	2d contact at	8	46	54	

Conſidering the quickneſs, with which the luminous thread of the ſun's limb was broken, by the approach of Venus's limb, I have ſufficient foundation for ſuppoſing, that almoſt all the difference between theſe obſervations, for the firſt contact, depends ſolely upon the different goodneſs of the inſtruments, and particularly the meaſuring the time. It ſeems to me, that the obſervations of the laſt contact agree, for the ſame reaſons that the others differ from one another.

Being in company laſt night at ſupper with Mr. de la Lande *, I had the pleaſure of hearing him do the Engliſh nation juſtice, with regard to what the learned world owe it; expreſſing, at the ſame time, a very great deſire of ſeeing England, as ſoon as peace is eſtabliſhed between it and his own country. In the mean time, he preſents his obſervations upon the paſſage of Venus to the Royal Society, as a teſtimony

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* Of the Royal Academy of Sciences.

of his reſpect for that illuſtrious mother of the ſciences.

I beg, Sir, you will be ſo obliging as to ſend me whatever obſervations are made in England upon this paſſage, under a cover, directed to Monſ. De la Lande, who, after copying them, will ſend them to me, where I ſhall direct him.

I am juſt upon quitting Paris, to go into Italy, after viſiting the provinces of France. You will oblige me very much, Sir, if you will preſent my moſt humble reſpects to all the Gentlemen of the Royal Society, to whom I have the honour of being known; and am,

With the moſt perfect conſideration,

S I R,

Your moſt humble

and obedient ſervant,

Paris,
June 20, 1761.

B. Ferner.

P. S. I hope Monſ. Baudouin's pieces upon the ſatellite of Venus is come to your hands. Notwithſtanding all the care taken here, to diſcover this ſatellite upon the diſk of the ſun, on the 6th paſt, we could ſee nothing of it.

XLII. *Obſervations on the ſame Tranſit of* Venus *made at* Conſtantinople: *By his Excellency* James Porter, *Eſq*; *his Majeſty's Ambaſſador there*: *In a Letter to* George Amyand, *Eſq*;

Read Nov. 19, 1761.

I Have not time to write to any of the Royal Society. Pray tell Mr. Burrew, with my compliments, Father Boſcowitz will be here only in September, or October, with the new Venetian ambaſſador.

The ſun riſes at Stanbole, Conſtantinople, the 6th June, $4^h\ 32'$.

Venus entered the ſun much earlier, and is ſuppoſed to have entered its diſk above an hour and more, when ſeen at its riſing here.

Venus, at emerging out of the ſun's diſk, touched the interior limb of the ſun S. E. at $10^h\ 15'$.

Emerged totally the point of contact, at its going out, at $10^h\ 32'\ 20''$.

Obſerved with a Hadley's reflector 18 inches only, and a good pendulum, with ſeconds.

Yours,

J. P.

XLIII. *An Account of the Obſervations made on the ſame Tranſit at* Upſal *in* Sweden: *In a Letter to Mr.* Benjamin Wilſon, *F. R. S. from Mr.* Thorbern Bergman, *of* Upſal.

Ampliſſime atque Celeberrime Domine,

Read Nov. 19, 1761.

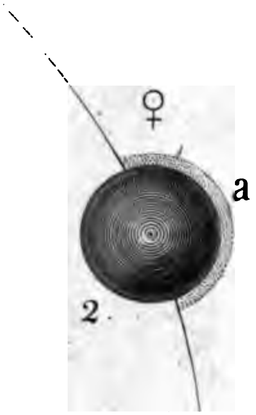
QUamvis adhuc neſciam, utrum mea epiſtola, menſe Majo ſcripta, in tuas pervenerit manus, necne; interim tamen juſtæ mihi jam adſunt ſcribendi rationes. Accepi nimirum, menſe Junio, tuum librum de electricitate à Domino Ferner, uti tuum donum tranſmiſſum, pro quo gratias ago debeoque maximas.

Voluptate eum perlegi; optans velles, in nova editione, recentiora tua tentamina, quæ Tranſ. Philoſoph. inſerta ſunt, addere; adeo ut ſimul haberi poſſent tua experimenta, quæ jam ſparſa quærere neceſſum eſt.

Ad Veneris tranſitum per diſcum Solis, die 6 Junii, rite obſervandum, aſtronomi Upſalenſes, diebus antecedentibus, omni cura et ſtudio, cuncta in obſervatorio, quæ huic fini inſervire poſſent, præpararunt; omniaque, favente cœlo, dicto die feliciter peracta ſunt. D^us Profeſſor Strömer tubo aſtronomico viginti pedum introitum et exitum contemplatus eſt; Dominus Obſervator Mallet uſus eſt catadioptrico octodecim digitor. Angl. inſtructo Dollondi micrometro objectivo, cujus diſtantia focalis eſt triginta ped. Angl. ego vero teleſcopium aſtronomicum adhibui 21 ped. Suec. ſeu circ. 19 ped. Gallic. cujus vitrum oculare habet diſtantiam focalem

2.9 dig. Suec. Per vitrum rubrum tenue, ingreſſum, egreſſum autem, per craſſius vidi. Hâc occaſione phænomena nonnulla minus expectata adparuerunt. Imprimis, Venerem atmoſphæra circumdatam obſervaſſe credimus; ſequentibus nixi rationibus. Scilicet, ante completam immerſionem, ſeu adhuc quarta circiter parte diametri Veneris extra marginem Solis exiſtente, tota Venus viſa eſt; nam pars extra prominens debili lumine erat cincta, uti Fig. 1. monſtrat. [*Vide Tab.* VIII.] Hoc vero longe clarius ſub emerſione notatum fuit; etenim, initio, partem extra Solis marginem prominentem ſimile lumen, ſed clarius circumdedit, cujus tamen particula *a* (Fig. 2.) à Sole maxime diſtans, eo magis fuit debilitata, quo magis evaſit Venus; adeo ut tandem non niſi cornua (Fig. 3.) conſpici poſſent. Interim tamen, uſque dum centrum Veneris egreſſum erat, lumen hocce integrum videbam. Aliud præterea phænomenon adnotatum fuit; quod forſan ex valida radiorum Solis refractione, in tranſitu per atmoſphæram Veneris, explicari poteſt. Circa contactum interiorem, ſeu dum limbus Veneris à Solis interiore ſepararetur, hoc non momento evenit; ſed haud aliter ac binæ guttæ aqueæ, ſeparandæ inter ſe, ligamentum formant, ita quoque è Venere ad marginem Solis tuberculum nigrum extendebatur (Fig. 4.) Hoc vero ligamento tandem in medio rumpente, momento adparuit diſtantia limbi proximi Veneris à margine Solis æqualis circiter octavæ parti diametri Veneris. Circa emerſionem eadem fere adparentia occurrebat, quamvis non adeo diſtincte, et in contactu ultimo ligamento quaſi Soli cohærebat Venus (Fig 5.)

Philos. Trans. Vol. LII. TAB. VIII. *p.* 228.



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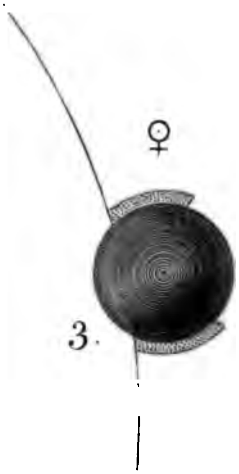

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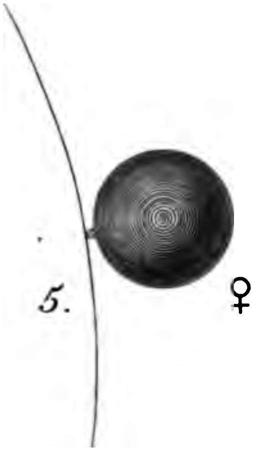

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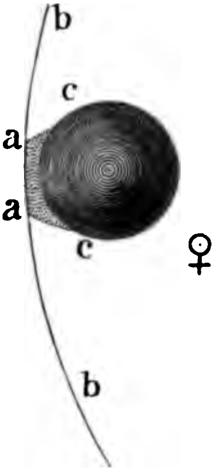


6.

J. Mynde sc.

Momenta, quæ circa immerſionem et emerſionem adnotavi, quæque à reliquorum obſervationibus parum differunt, ſunt ſequentia.

Tempus aſtronomicum verum die 5 Junii.

	Hor.	Min.	Sec.
Primum adpulſum ad Solis marginem obſervare impoſſibile fuit, nec ullam marginis excavationem vidi ante - - - - - - -	15	21	
Cornua Solis *bac*, *bac*, (Fig. 6.) tantummodo debili atmoſphæræ lumine *aa* ſeparata vidi - -	15	37	28
Contactum interiorem, ſeu momentum quo cornua *bac*, *bac*, confluebant æſtimavi - - - -	15	37	43
Venus egrediens Solem interne tangere, ſeu ejus marginem aperire mihi viſa eſt - - - - - -	21	28	9
Contactum vero exteriorem, ſeu ultimum Veneris veſtigium in margine Solis vidi - - - - - -	21	46	30

Obſervationes ſub ipſo tranſitu ad orbitam determinandam factas jam prætereo, hoc tantum addens; Dominum Mallet ſat adcurate definiviſſe diametrum Veneris inter 57 et 58 ſecunda. Præterea in variis intra regnum locis rariſſimum hocce ſpectaculum feliciter obſervatum eſt, imprimis Stockholmiæ.

In Suecia notum eſt meteoron quoddam, lingua vernacula Kornbleck dictum, quod fulgurationem hordei ſignificat. Adparet nimirum veſpertino tempore, præcipue menſis Auguſti, dum hordeum maturari incipit; unde denominatio ſumta eſt. Conſiſtit autem hoc in fulguratione quadam tacita abſque ullo inſequente murmure vel tonitru, cœlo plerumque omnimodo ſereno exiſtente. Diu incertum fuit quid ſint; æſtate vero proxime præterlapſa, didici, à fulgure tonante vix eſſe diſtincta.

Semper enim, dum ejuſmodi fulgurationes hordei viſæ fuerint, alicubi, juxta horizontem, nubeculam detegere potui, ex qua oriebantur. Deinde etiam comperi, eodem tempore tonaſſe, ad magnam diſtantiam, in ea plaga ubi nubes eſſet obſervata. Itaque in eo tantum à communi fulgure differre videntur, quod ob diſtantiam obmuteſcant. Præterea nullus dubito, quin die ſæpe fiant, quamvis fortiore lumine diſpareant. Fieri quoque poteſt, ut interdum nubes fulgurans, paululum infra horizontem natans, nihilominus, per lumen reflexum, fulgurationes ſupra eandem cauſſetur.

Una vel altera vice pluviam veſperi cadentem adeo electricam obſervavi, ut ad ejus contactum omnia ſcintillarent; terraque undis quaſi igneis obtegeretur; abſque tamen omni tonitru. Idem interdum nive accidit. Eſt præterea aliud fulguris muti genus, quod ſero autumno et hieme conſpicitur, cujus ortus difficilius explicatur; ſaltem alio modo procreari videtur quam tonans; nam tonitrua hieme ignoramus.

Hæc phænomena obſervavi, et dum cœlum ſerenum, et dum nubibus omnino erat tectum. Quantum vero adhuc ex propriis obſervationibus colligere poſſum, ultra nubium regionem elevata ſunt. Sed de his alia occaſione plura. Interea mihi favere pergas. Permaneo

Celeberrimi nominis tui

cultor obſervatiſſimus,

Dabam Upſalæ,
die 28 Auguſti 1761.

Thorbern Bergman.

XLIV. *An Account of the Obſervations made upon the Tranſit of* Venus *over the Sun,* 6*th* June 1761, *at* Cajaneburg *in* Sweden, *by Monſ.* Planman: *Communicated in a Letter from Mr.* Peter Wargentin, *Secretary to the Royal Academy of Sciences in* Sweden, *and F. R. S. to Mr.* John Ellicott, *F. R. S. Tranſlated from the* French.

Stockholm, October 30, 1761

Read Nov. 26, 1761.

IN a letter, of the 7th of Auguſt, which I wrote to you, I communicated to you ſome obſervations on the late tranſit of Venus, made in Sweden, and, among theſe, that of Monſ. Planman, made at Cajanebourg. Since that time, ſome new obſervations have ſhewn, that the difference of meridians between Stockholm and Cajanebourg is only 38′ 40″ to 45″, that is, half a minute leſs than I preſumed then. This correction has convinced Monſ. Planman, that, in his obſervation on the interior contact of the limbs of Venus and the ſun, there has happened an error of a minute; not through the fault of the obſerver, but that of his aſſiſtant, who counted the ſeconds at the clock: ſo that, inſtead of 10^h 8′ 58″, it ſhould be 10^h 7′ 58″. I think it extremely probable, that this is the fact; for, upon this correction, all the obſervations of Monſ. Planman agree very well with each other, and with thoſe of the other aſtronomers.

XLV. *A ſecond Letter to the Right Hon. the Earl of* Macclesfield, *Preſident of the Royal Society, concerning the Tranſit of* Venus *over the Sun, on the 6th of* June 1761; *by the Rev.* Nathanael Bliſs, *M. A.* Savilian *Profeſſor of Geometry in the Univerſity of* Oxford, *and F. R. S.*

My Lord,

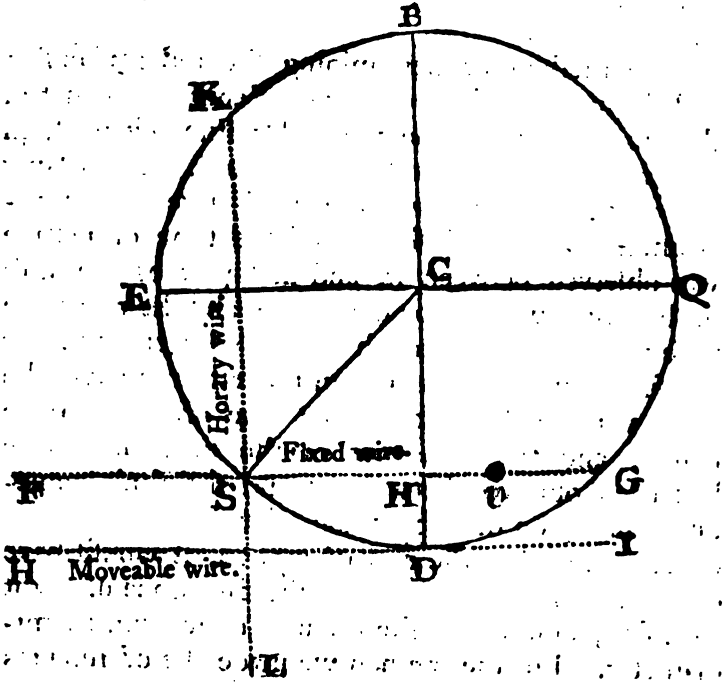
Read Jan 7, 1762. THE interior conjunctions of the planets Mercury and Venus, that happen near the ecliptic limits, have always engaged the attention of aſtronomers, as they furniſh the beſt means of determining ſome of the moſt important elements in the theory of thoſe Planets. The tranſits of the former have been often and carefully obſerved by the moſt eminent aſtronomers, ever ſince the invention of the teleſcope; and, it may be preſumed, that the elements of Mercury's theory are eſtabliſhed as accurately as can be expected. The opportunities of obſerving Venus upon the ſun's diſk occur ſo ſeldom, that the aſtronomers of theſe days have reaſon to think themſelves peculiarly happy, in being eye-witneſſes of ſo rare a phænomenon; more particularly too, as the advantages reſulting from the obſervations of this tranſit, are, in all probability, of the greateſt moment. The firſt, and only obſervation of this kind, was made by our ingenious countryman, the Rev. Mr. Jeremiah Horrox, a young gentleman of very diſtinguiſhed abilities, who, by his own obſervations, with inſtruments conſtructed under his own inſpection, and finiſhed by his own hands, was enabled to correct the ſo much boaſted tables of Lanſbergius, and to predict, with a degree of preciſion unknown to thoſe times, a phænomenon, which he himſelf thought to be of great conſequence. He immediately communicated this important diſcovery to his friend, and companion in his aſtronomical ſtudies, Mr. William Crabtree, and earneſtly exhorted him to prepare for the obſervation. The ſtate of the heavens, on that day, was not very favourable: however, both Mr. Horrox and his friend were lucky enough to obſerve it; the former, at a time when the limbs of the ſun and Venus were in the point of contact, viz. on the 24th of November 1639, O. S. And theſe two were the firſt, and only perſons, that ever ſaw Venus in the ſun, before the preſent year.

By the Rudolphine tables, conſtructed from the obſervations of Tycho Brahe, Kepler was enabled to predict, in the year 1629, that Venus would paſs over the ſun's diſk in the year 1761: and my worthy predeceſſor, that eminent aſtronomer and mathematician, Dr. Halley, in a memoir publiſhed in the Philoſophical Tranſactions, N° 348. exhorted the aſtronomers of all countries to attend to this rare phænomenon, with all poſſible diligence; as it would furniſh them with the beſt means of determining the parallax and diſtance of the ſun, and, conſequently, the dimenſions of the whole ſolar ſyſtem. How far the method propoſed by him, will enable us to ſolve this difficult problem, muſt be left to time to diſcover, when the obſervations, made in places properly ſituated, can be compared with thoſe made here, and in other famous obſervatories. The attention paid to the opinion of an Engliſh aſtronomer, by the moſt renowned Princes, more particularly by his late Majeſty, at the requeſt of your Lordſhip, and the Royal Society, will reflect the greateſt honour upon their names, to the lateſt poſterity. But as the tables, which Dr. Halley made uſe of, were very imperfect, his own not being then conſtructed, and did not repreſent the place of Venus on the ſun with that accuracy, which the method, in this caſe, required: and as that eminent philoſopher committed a ſmall miſtake in his calculations, by placing the axis of Venus's path, and the axis of the equator, on the ſame ſide of the axis of the ecliptic; a miſtake which the moſt accurate calculator might eaſily fall into: from theſe conſiderations, I ſay, the honour of determining the ſun's true parallax is, probably, reſerved for the reign of his preſent Majeſty; from whom, as a patron of ſcience, and every uſeful art, we have the greateſt reaſon to promiſe ourſelves every poſſible encouragement and aſſiſtance.

I have already had the honour of preſenting to your Lordſhip, and the Royal Society, an account of the obſervations of the contacts of the ſun's and Venus's limbs, made at Greenwich, and at your Lordſhip's own obſervatory. As the time would not then permit me to examine the obſervations made with the micrometer, I could only ſelect a few particulars, relating to the diameters of the ſun and Venus, as meaſured by different obſervers. I have ſince had leiſure to examine all the obſervations made upon the day of the tranſit, both at Shirburn caſtle, and at the Royal Obſervatory at Greenwich; and ſhall now beg leave to lay before you, both the obſervations themſelves, and the ſeveral reſults deducible from them by calculation.

The method of determining the right aſcenſion and declination of the center of Venus from that of the ſun, was the ſame which Dr. Bradley uſed, in obſerving a former tranſit of Mercury. The planet was made to run down the fixed wire of the micrometer, and the difference of the time of paſſage was obſerved between it, and that part of the ſun's limb, which was cut by that wire; and the moveable wire was brought to touch the ſun's lower limb. If the ſun's lower limb had been made to run down the fixed wire, and the moveable wire brought to the planet, and the difference of the time of paſſage had been obſerved between it and the ſun's conſequent limb, on the ſuppoſition, that the wire was not exactly parallel to the diurnal motion, it would have cauſed a conſiderable error in the difference of right aſcenſion, obſerved at the diſtance of the ſun's ſemidiameter. But the method we made uſe of requires ſome calculation, to determine the poſition of Venus on the ſun's diſk.

FIG. 1.



Let, therefore, in Fig. 1. the circle EDQB repreſent the ſun's diſk, in which let EQ be parallel to the equator, and BD an hour-circle; let the pricked line FG repreſent the fixed wire of the micrometer, HI the moveable wire, and KL the perpendicular or horary wire. The difference of right aſcenſion Hv, and of declination CH, will be determined in the following manner: SC, or CD, the ſemidiameter of the ſun is given, and CD — DH = CH, the difference of declination; and SC and CH being given, SH may be found; and then the obſerved difference of right aſcenſion Sv being diminiſhed in the ratio of radius to the ſine of the polar diſtance of Venus, will give HU, the difference of right aſcenſion.

As the clouds began to diſperſe, and the ſky to become favourable, at Shirburn caſtle, above two hours before we had any opportunity of obſerving at Greenwich, I ſhall firſt give the obſervations there made by Mr. Hornſby, and afterwards my own at Greenwich. But here I would beg leave to premiſe, that, though the numbers are given to parts of a ſecond, the obſervers do not pretend to an imaginary exactneſs, (for they did not eſtimate the times of paſſage nearer than a quarter of a ſecond of time) but the numbers are ſuch as reſult from the turning minutes and ſeconds of time into motion, and the revolutions and parts of the ſcrew of the micrometer into minutes and ſeconds.

The ſun's horizontal diameter, as meaſured by the micrometer, was 31′ 33″, and that of Venus, by ſeveral obſervers, 58″; the following obſervations were therefore reduced, by aſſuming the ſemidiameter of the ſun = 15′ 46″.5, and that of Venus = 29″.

1. At 17^h 33′ 50″, apparent time, at Shirburn, the center of Venus preceded the part of the ſun's limb, cut by the fixed wire, 12′ 34″ in motion; and the north, or upper limb of Venus, was north of the ſouthern, or lower limb of the ſun, 6′ 29″.6; therefore, the center of the ſun preceded the center of Venus in right aſcenſion 1′ 36″.9; and the center of Venus was ſouth of that of the ſun in declination 9′ 45″.8. The ſame, to avoid repetition, in all the following obſervations.

2. At 17^{h} 35′ 41″, the center of Venus preceded the ſun's limb 12′ 13″; and the upper limb of Venus was north of the ſun's lower limb 6′ 19″.7: ſun's center, therefore, before that of Venus in right aſcenſion, 1′ 21″; and the center of Venus was ſouth of the ſun's center in declination 9′ 55.7″.

3. At 17^{h} 40′ 1″, Venus before ſun's limb 12′ 37½″, and was north of ſun's lower limb 6′ 18.2″: therefore, ſun's center, before that of Venus in right aſcenſion, 57″.2; and Venus ſouth of ſun's center in declination 9′ 57″.3.

N. B. In theſe obſervations, the ſun's limb undulated.

4. At 17^{h} 43′ 59″, Venus before ſun's limb 12′ 47″; and was north of ſun's lower limb 6′ 16″.3: therefore, ſun's center before Venus in right aſcenſion 47″; and Venus ſouth of ſun's center in declination 9′ 59″.2.

5. At 17^{h} 50′ 31″, Venus before ſun's limb 13′ 9¼″; and was north of ſun's lower limb 6′ 10″.4: therefore, ſun's center before Venus in right aſcenſion 22″; and Venus ſouth of ſun's center in declination 10′ 5″.1.

6. At 18^{h} 3′ 41″, Venus before ſun's limb 13′ 48¾″; and was north of ſun's lower limb 5′ 59″.2: therefore, the center of Venus was before the ſun's center in right aſcenſion 23″.2; and was ſouth of ſun's center in declination 10′ 16″.3.

7. At 18^{h} 8′ 54″, Venus before ſun's limb 14′ 13″; and was north of ſun's lower limb 5′ 53″.8: therefore, Venus before ſun's center in right aſcenſion 49″.9; and was ſouth of ſun's center in declination 10′ 21″.7.

8. At 18ʰ 15′ 50″, Venus before ſun's limb 14′ 33″; and was north of ſun's lower limb 5′ 48″: therefore, Venus before ſun's center in right aſcenſion 1′ 13″.2; and was ſouth of ſun's center 10′ 27″.5 in declination.

9. At 18ʰ 28′ 6″, Jenus before ſun's limb 15′ 9¼″; and was north of ſun's lower limb 5′ 35″.4: therefore, Venus before ſun's center in right aſcenſion 1′ 57″.2; and was ſouth of ſun's center in declination 10′ 40″.1.

10. At 19ʰ 18′ 49″, Venus before ſun's limb 18′ 5½″; and was north of ſun's lower limb 4′ 45″.5: therefore, Venus before ſun's center in right aſcenſion 5′ 25″.3; and was ſouth of ſun's center 11′ 30″.

11. At 19ʰ 22′ 37″, Venus before ſun's limb 18′ 9½″; and was north of ſun's lower limb 4′ 44″.3: therefore, Venus before ſun's center in right aſcenſion 5′ 29″.9; and was ſouth of ſun's center 11′ 31″.2.

12. At 19ʰ 25′ 50″, Venus before ſun's limb 18′ 23¾″; and was north of ſun's lower limb 4′ 42″.5: therefore, Venus before ſun's center in right aſcenſion 5′ 45″; and was ſouth of ſun's center 11′ 32″.9.

13. At 19ʰ 29′ 20″, Venus before ſun's limb 18′ 31¾″; and was north of ſun's lower limb 4′ 35″.2: therefore, Venus before ſun's center in right aſcenſion 5′ 59″.8; and was ſouth of ſun's center in declination 11′ 40″.3.

14. At 19ʰ 45′ 58″, Venus before ſun's limb 19′ 20¼″; and was north of ſun's lower limb 4′ 19″: therefore, Venus before ſun's center in right aſcenſion 7′ 1″.5; and was ſouth of ſun's center 11′ 56″.5.

15. At 19ʰ 49′, Venus before ſun's limb 19′ 28″; and was north of ſun's lower limb 4′ 16″.6: therefore,

fore, Venus before ſun's center in right aſcenſion 7′ 11″.1; and was ſouth of ſun's center 11′ 58″.9.

16. At 20h 12′ 1′, the center of Venus followed the ſun's preceding limb, cut by the fixed wire, 1′ 58″; and was north of ſun's lower limb 3′ 54″.2: therefore, Venus before ſun's center in right aſcenſion 8′ 34″.2; and was ſouth of ſun's center in declination 12′ 21″.3.

The following obſervations were made by myſelf, at Greenwich, as ſoon as the ſky became favourable.

1. At 19h 38′ 21″, apparent time, at Greenwich, the antecedent, or firſt limb of Venus, preceded that part of the ſun's limb cut by the fixed wire 18′ 48½″ in motion; and the center of Venus was north of the ſouthern, or lower limb of the ſun, 4′ 4″.5: therefore, the center of Venus preceded the ſun's center in right aſcenſion 6′ 18″.9; and was ſouth of that of the ſun in declination 11′ 42″.1.

2. At 19h 42′ 9″, the limb of Venus before ſun's limb 18′ 52½″; and was north of ſun's lower limb 3′ 56″.8: therefore, Venus before ſun's center in right aſcenſion 6′ 31″; and was ſouth of it in declination 11′ 49″.7. But this is marked as dubious.

3. At 19h 44′ 35″, limb of Venus before ſun's limb 19′; and center was north of ſun's lower limb 3′ 57″.5: therefore, Venus before ſun's center in right aſcenſion 6′ 37″.1; and was ſouth of ſun's center in declination 11′ 49″.

4. At 19h 53′ 14″, limb of Venus before ſun's limb 19′ 22½″; and was north of ſun's lower limb 3′ 51″.3: therefore, Venus before ſun's center in right aſcenſion 7′ 5″; and was ſouth of ſun's center in declination 11′ 55″.2.

5. At $19^{h}\ 58'\ 26''$, limb of Venus before ſun's limb $19'\ 37\frac{1}{2}''$; and was ſouth of ſun's lower limb $3'\ 43''$: therefore, Venus before ſun's center in right aſcenſion $7'\ 28''.6$; and was ſouth of ſun's center in declination $12'\ 3''.5$.

The few obſervations, which were afterwards made by Mr. Green, with Mr. Dollond's micrometer, are omitted; for they diſagree ſo much with themſelves, and alſo with the above, that there muſt be ſome error in reading the numbers of the nonius; or, which is more probable, in placing the micrometer exactly parallel to the equator, occaſioned by the hurry with which they were made.

In order to determine more exactly the time of the ecliptic conjunction, with the latitude of Venus then; together with the time of the middle of the tranſit, and the neareſt approach of the centers; and from thence the true place of her node; I have carefully computed the following numbers from theory: becauſe, as Dr. Halley has obſerved, in the Philoſophical Tranſactions, N° 386, “ there is always an unavoid-
“ able, though ſmall uncertainty in what we obſerve,
“ yet greater than there can be in the theory, eſpe-
“ cially now it is ſo very near the truth.” The ſolar numbers were computed from new tables, not yet publiſhed, corrected by the ſmall equations, occaſioned by the influence of the moon and planet Jupiter, and alſo the nutation of the earth's axis. The ſun's place was very well obſerved on the meridian, both at Greenwich and Shirburn, the day of the tranſit; which, allowing for the difference of longitude of thoſe places, agreed to a ſurprizing exactneſs, within two ſeconds; and did not differ more than five

fulguratione quadam tacita abſque ullo inſequente murmure vel tonitru, cœlo plerumque omnimodo ſereno exiſtente. Diu incertum fuit quid ſint; æſtate vero proxime præterlapſa, didici, à fulgure tonante vix eſſe diſtincta.

Semper enim, dum ejuſmodi fulgurationes hordei viſæ fuerint, alicubi, juxta horizontem, nubeculam detegere potui, ex qua oriebantur. Deinde etiam comperi, eodem tempore tonaſſe, ad magnam diſtantiam, in ea plaga ubi nubes eſſet obſervata. Itaque in eo tantum à communi fulgure differre videntur, quod ob diſtantiam obmuteſcant. Præterea nullus dubito, quin die ſæpe fiant, quamvis fortiore lumine diſpareant. Fieri quoque poteſt, ut interdum nubes fulgurans, paululum infra horizontem natans, nihilominus, per lumen reflexum, fulgurationes ſupra eandem cauſſetur.

Una vel altera vice pluviam veſperi cadentem adeo electricam obſervavi, ut ad ejus contactum omnia ſcintillarent; terraque undis quaſi igneis obtegeretur; abſque tamen omni tonitru. Idem interdum nive accidit. Eſt præterea aliud fulguris muti genus, quod ſero autumno et hieme conſpicitur, cujus ortus difficilius explicatur; ſaltem alio modo procreari videtur quam tonans; nam tonitrua hieme ignoramus.

Hæc phænomena obſervavi, et dum cœlum ſerenum, et dum nubibus omnino erat tectum. Quantum vero adhuc ex propriis obſervationibus colligere poſſum, ultra nubium regionem elevata ſunt. Sed de his alia occaſione plura. Interea mihi favere pergas. Permaneo

Celeberrimi nominis tui

cultor obſervatiſſimus,

Dabam Upſalæ,
die 28 Auguſti 1761.

Thorbern Bergman.

XLIV. *An Account of the Obſervations made upon the Tranſit of* Venus *over the Sun,* 6*th* June 1761, *at* Cajaneburg *in* Sweden, *by Monſ.* Planman: *Communicated in a Letter from Mr.* Peter Wargentin, *Secretary to the Royal Academy of Sciences in* Sweden, *and F. R. S. to Mr.* John Ellicott, *F. R. S. Tranſlated from the* French.

Stockholm, October 30, 1761

Read Nov. 26, 1761.

IN a letter, of the 7th of Auguſt, which I wrote to you, I communicated to you ſome obſervations on the late tranſit of Venus, made in Sweden, and, among theſe, that of Monſ. Planman, made at Cajanebourg. Since that time, ſome new obſervations have ſhewn, that the difference of meridians between Stockholm and Cajanebourg is only 38′ 40″ to 45″, that is, half a minute leſs than I preſumed then. This correction has convinced Monſ. Planman, that, in his obſervation on the interior contact of the limbs of Venus and the ſun, there has happened an error of a minute; not through the fault of the obſerver, but that of his aſſiſtant, who counted the ſeconds at the clock: ſo that, inſtead of $10^h\ 8'\ 58''$, it ſhould be $10^h\ 7'\ 58''$. I think it extremely probable, that this is the fact; for, upon this correction, all the obſervations of Monſ. Planman agree very well with each other, and with thoſe of the other aſtronomers.

XLV. *A*

in other famous obſervatories. The attention paid to the opinion of an Engliſh aſtronomer, by the moſt renowned Princes, more particularly by his late Majeſty, at the requeſt of your Lordſhip, and the Royal Society, will reflect the greateſt honour upon their names, to the lateſt poſterity. But as the tables, which Dr. Halley made uſe of, were very imperfect, his own not being then conſtructed, and did not repreſent the place of Venus on the ſun with that accuracy, which the method, in this caſe, required: and as that eminent philoſopher committed a ſmall miſtake in his calculations, by placing the axis of Venus's path, and the axis of the equator, on the ſame ſide of the axis of the ecliptic; a miſtake which the moſt accurate calculator might eaſily fall into: from theſe conſiderations, I ſay, the honour of determining the ſun's true parallax is, probably, reſerved for the reign of his preſent Majeſty; from whom, as a patron of ſcience, and every uſeful art, we have the greateſt reaſon to promiſe ourſelves every poſſible encouragement and aſſiſtance.

I have already had the honour of preſenting to your Lordſhip, and the Royal Society, an account of the obſervations of the contacts of the ſun's and Venus's limbs, made at Greenwich, and at your Lordſhip's own obſervatory. As the time would not then permit me to examine the obſervations made with the micrometer, I could only ſelect a few particulars, relating to the diameters of the ſun and Venus, as meaſured by different obſervers. I have ſince had leiſure to examine all the obſervations made upon the day of the tranſit, both at Shirburn caſtle, and at the Royal Obſervatory at Greenwich; and ſhall now beg leave to lay before you, both the obſervations themſelves, and the ſeveral reſults deducible from them by calculation.

The method of determining the right aſcenſion and declination of the center of Venus from that of the ſun, was the ſame which Dr. Bradley uſed, in obſerving a former tranſit of Mercury. The planet was made to run down the fixed wire of the micrometer, and the difference of the time of paſſage was obſerved between it, and that part of the ſun's limb, which was cut by that wire; and the moveable wire was brought to touch the ſun's lower limb. If the ſun's lower limb had been made to run down the fixed wire, and the moveable wire brought to the planet, and the difference of the time of paſſage had been obſerved between it and the ſun's conſequent limb, on the ſuppoſition, that the wire was not exactly parallel to the diurnal motion, it would have cauſed a conſiderable error in the difference of right aſcenſion, obſerved at the diſtance of the ſun's ſemidiameter. But the method we made uſe of requires ſome calculation, to determine the poſition of Venus on the ſun's diſk.

FIG.

puted viſible motion in longitude and latitude, by making the proper proportion, the viſible ecliptic conjunction will be found at 17^h 40′ 3″ apparent time, at Shirburn, or at 17^h 44′ 4″ apparent time, at Greenwich, when the viſible latitude was 9′ 57″.6 ſouth of the ſun's center. At 17^h 56′ 46″, the parallax in longitude (ſuppoſing, as above, the horizontal parallax of the ſun to be $10\frac{1}{3}''$) will be 14″, to be added to the viſible longitude of Venus, to give her true longitude before the ſun's center, and 20″.5 to be ſubtracted from the viſible latitude, to give the true latitude, as ſeen from the center of the earth. The true ecliptic conjunction, therefore, was at 17^h 36′ 25″ apparent time, at Sirburn, or at 17^h 40′ 26″ apparent time, at Greenwich, by making a proper proportion from the computed true motion of Venus from the ſun; and the true latitude was then 9′ 34.5″ ſouth.

2. From the mean of 10th, 11th, 12th, and 13th obſervations, at 19^h 24′ 9″ apparent time, at Shirburn, the obſerved right aſcenſion was 5′ 40″, and the obſerved declination was 11′ 33.6″; from whence the viſible longitude was 6′ 52.2″, and the viſible latitude 10′ 53″.3, from the ſun's center; and the viſible ecliptic conjunction was at 17^h 40′ 23″, at Shirburn, or at 17^h 44′ 24″ apparent time, at Greenwich, with 9′ 54.9″ of viſible latitude ſouth. The parallax of longitude was 13.2″, to be added to the viſible longitude; and the parallax of latitude 18.1″, to be ſubtracted from the viſible latitude, to give the true latitude. The true ecliptic conjunction was, therefore, at 17^h 36′ 31″, at Shirburn, or at

$17^{h}\ 40'\ 32''$ apparent time, at Greenwich; the true latitude being then $9'\ 31''.6$ ſouth.

3. From the mean of the 14th and 15th obſervations, at $19^{h}\ 47'\ 29''$, the obſerved right aſcenſion was $7'\ 6''.3$, and the obſerved declination $11'\ 57''.6$; from whence the viſible longitude was $8'\ 20''.5$, and the viſible latitude was $11'\ 8''$, from the ſun's center; and the viſible ecliptic conjunction was at $17^{h}\ 41'\ 30''$, or at $17^{h}\ 45'\ 31''$, apparent time, at Greenwich, with viſible latitude $9'\ 57''.2$ ſouth. The parallax of longitude was $12''.5$, to be added; and the parallax of latitude $17''.4$, to be ſubtracted, to give the true longitude and latitude. The true ecliptic conjunction was, therefore, at $17^{h}\ 37'\ 42''$, at Shirburn, or at $17^{h}\ 41'\ 43''$, apparent time, at Greenwich; the true latitude being then $9'\ 33''.9$ ſouth.

4. At the internal contact, at Shirburn, at $20^{h}\ 15'\ 10''$, if the motion in declination, anſwering to $3'$ of time, be added to the declination obſerved at the 16th obſervation, the declination of the center of Venus from the ſun's center will be $12'\ 24''.4$; from whence the viſible longitude was $10'\ 12''.6$, and the viſible latitude $11'\ 23''$, from the ſun's center; and the viſible ecliptic conjunction was at $17^{h}\ 40'\ 57''$, at Shirburn, or at $17^{h}\ 44'\ 58''$, apparent time, at Greenwich, with $9'\ 56''.2$ of viſible latitude ſouth. The parallax of longitude, to be added, was $11''.6$; and the parallax of latitude $16''.5$, to be ſubtracted, to give the true longitude and latitude. The true ecliptic conjunction was, therefore, at $17^{h}\ 37'\ 13''$, at Shirburn, or at $17^{h}\ 41'\ 14''$, apparent time, at Greenwich; the true latitude being $9'\ 33''.1$ ſouth.

5. The 2d obſervation made at Greenwich being dubious, if the mean of the 1ſt, 3d, 4th, and 5th, be taken at $19^{h}\ 48'\ 39''$, apparent time, at Greenwich, the obſerved right aſcenſion was $6'\ 52''.4$, and the obſerved declination $11'\ 52''.4$; from whence the viſible longitude was $8'\ 6''.1$, and the viſible latitude $11'\ 4''.3$, from the ſun's center. The viſible ecliptic conjunction was, therefore, $17^{h}\ 46'\ 17''$, apparent time, at Greenwich, with $9'\ 55''.5$ of viſible ſouth latitude. The parallax of longitude, to be added, was $12''.6$; and the parallax of latitude, to be ſubtracted, $17''.3$, to give the true longitude and latitude from the ſun's center. The true ecliptic conjunction, therefore, was at $17^{h}\ 42'\ 28''$, apparent time, at Greenwich; when the true latitude was $9'\ 32''.4$.

I have omitted the computation of the longitude, latitude, and of the viſible and true conjunction from the internal contact, at Greenwich, and the difference of declination, as given in my laſt letter; becauſe there muſt have been ſome miſtake in reading the numbers of the micrometer, or in ſetting them, or the times, down: for they differ too much from all the above, which correſpond ſo well with each other, (though made at different places, and with different inſtruments) and give the true latitude, at the ecliptic conjunction, about $8''$ leſs, that we cannot ſafely depend upon them.

If, therefore, we ſuppoſe the viſible ecliptic conjunction to have happened at $17^{h}\ 45'\ 3''$, apparent time, at Greenwich, being the mean of the five foregoing deductions, where the greateſt difference is no more than $2'\ 13''$ of time, or $8''$ of viſible longitude, with 9′ 56″.3 of viſible ſouth latitude, from the ſun's center; where the greateſt difference is no more than 2″.7 in latitude, we cannot much err from the truth: and alſo, from the mean of the ſame deductions, the true ecliptic conjunction, as ſeen from the earth's center, will be at 17^{h} 41′ 17″, with 9′ 33″.1 of ſouth latitude. The middle of the tranſit was, therefore, at 17^{h} 20′ 5″; and the neareſt approach of the centers 9′ 26″.8. The latitude then was 9′ 20″.6 ſouth; but the longitude of Venus being augmented by the aberration of light 3″.7, equivalent to 56″ of time, by which the true ecliptic conjunction was accelerated, the true equated conjunction was at 17^{h} 42′ 13″. The error in latitude, cauſed by the aberration of light, was 1″.4, by which it was diminiſhed; the equated latitude, therefore, was 9′ 34″.5.

The equation of time was then 1′ 52″, to be ſubtracted from the apparent time, to give the mean; conſequently, the true equated ecliptic conjunction, as ſeen from the earth's center, was at 17^{h} 40′ 21″, mean time, at Greenwich. The true place of the ſun, corrected by obſervation, was, at that time, 2^{s} 15° 36′ 12″; and, conſequently, the heliocentric place of Venus was 8^{s} 15° 36′ 12″, with the geocentric latitude 9′ 34″.5. Now, in this caſe, the geocentric latitude is to the heliocentric latitude, as the diſtance of Venus from the ſun is to the diſtance of Venus from the earth; and therefore, the planet's latitude, as ſeen from the ſun, was 3′ 48″.5. If we ſuppoſe the inclination of the orbit of Venus to be 3° 23′ 20″, as determined by Dr. Halley and M. Caſſini, the diſtance of Venus from the node will be

1° 4′ 20″;

$1^{\circ}\ 4'\ 20''$; conſequently, its true place $2^{s}\ 14^{\circ}\ 31'\ 52''$ on the day of the tranſit. The effect of refraction is not taken into theſe calculations; becauſe, at the firſt obſervations, when its effect would have been greateſt, it amounted only to a very ſmall part of a ſecond.

Theſe, my Lord, are the Concluſions, which I have been able to deduce, from the obſervations made at your Lordſhip's own obſervatory, and at the Royal Obſervatory at Greenwich. They are as faithfully related, as they were ſcrupulouſly calculated; and if they meet with the approbation of your Lordſhip, and of the Royal Society, I ſhall think myſelf ſufficiently rewarded, for the Labour of a long and tedious calculation.

I am,

With the greateſt reſpect,

My Lord,

Your Lordſhip's,

and the Royal Society's,

much obliged,

and moſt obedient,

humble ſervant,

Oxford,
Dec. 15, 1761.

Nathanael Bliſs.

XLVI. Ob-

XLVI. *Obſervatio tranſitus* Veneris *per diſcum* Solis, *facta Matriti die* 6ᵗ Junii 1761. *à* *P.* Antonio Eximeno, *è Soc. Jeſ.* *Communicated by* Cha. Morton, *M. D. Secretary.*

Read Jan. 14, 1762.

	°	'	"
SEcundum recentiſſimas obſervationes altitudo poli	40	25	0
Differentia temporaria reſpectu Pariſiorum	0	24	18

Facta eſt obſervatio cum quadrante duorum pedum cum dimidio, conſtructo à D. Georgio Adams; et cum horologio conſtructo à D. Ellicot.

	h	'	"	'''
Die 5ᵃ Junii verſabatur Sol in meridiano ſecundum altitudines correſpondentes, ad - - - - - -	11	54	35	0
Debuerat verſari ſecundum ephemeridas D. De la Caille, ad - - -	11	58	0	3
Die 6ᵃ ſecundum altitudines correſpondentes, erat Sol in meridiano ad	11	54	50	0
Debuerat eſſe ſecundum dictas ephemeridas, ad - - - - - - -	11	58	10	28

His elementis poterant corrigi tempora obſervationis; conſultius tamen viſum eſt incorrecta relinquere; ut quilibet poſſit illa corrigere iis elementis, quæ ipſi exactiora videantur. Sunt igitur tempora, quæ deinceps notabimus, quæ dabat horologium.

Cœpta eſt omnis obſervatio tangente limbo ſuperiore Solis filum horizontale micrometri, limbo verò dextero filum verticale. Tempora Veneris, quæ notantur, ſunt appulſus ipſius limbi dexteri ad filum verticale, et limbi ſuperioris ad horizontale.

Hæc Veneris tempora ſunt fortaſſe uno ſecundo juſto longiora: tremebat enim ſæpe pavimentum obſervatorii. Ego verò nunquam adnotavi tempus, niſi omnino ſecurus de appulſu; ſemper tamen antequam limbus Veneris per oppoſitam fili partem appareret.

Duplex obſervatio aſteriſmo notata, facta eſt armato oculo duplici ſimul vitro, altero cæruleo, fuliginoſo altero. Prima facta eſt nudo oculo; cæterarum partim vitro rubro, partim viridi.

Observatio 1ª.

	h	′	″
Sol ad utrumque filum	4	55	3
Venus ad verticale -		56	7
Venus ad horizontale		57	19
Sol ad horizontale -		58	7
Sol ad verticale - -		58	27

2ª.

Sol ad utrumque filum	5	5	46
Venus ad verticale -		6	54
Venus ad horizontale		8	1
Sol ad horizontale -		8	50
Sol ad verticale - -		9	22

3ª.

Sol ad utrumque filum		21	40
Venus ad verticale -		22	38
Venus ad horizontale		23	47
Sol ad horizontale -		24	40
Sol ad verticale		25	20

4ª.*

Sol ad utrumque filum		32	58
Venus ad verticale -		33	59
Venus ad horizontale		35	3
Sol ad horizontale -		35	56
Sol ad verticale - -		36	45

Observatio 5ª.

	h	′	″
Sol ad utrumque filum	5	40	54
Venus ad verticale -		41	54
Venus ad horizontale		42	54
Sol ad horizontale -		43	50
Sol ad verticale - -		44	47

6ª.

Sol ad utrumque filum		48	44
Venus ad verticale -		49	34
Venus ad horizontale		50	41
Sol ad horizontale -		51	38
Sol ad verticale - -		52	31

7ª.*

Sol ad utrumque filum	6	0	34
Venus ad verticale -		1	9
Venus ad horizontale		2	18
Sol ad horizontale		3	17
Sol ad verticale		4	14

8ª.

Sol ad utrumque filum		22	40
Venus ad verticale -		23	25
Venus ad horizontale		24	33
Sol ad horizontale -		25	39
Sol ad verticale - -		26	44

Obser-

Observatio 9ª.

Sol ad utrumque filum	6	33	56
Venus ad verticale -		34	35
Venus ad horizontale		35	40
Sol ad horizontale -		36	49
Sol ad verticale		37	57

10ª.

Sol ad utrumque filum		44	24
Venus ad verticale -		44	57
Venus ad horizontale		46	3
Sol ad horizontale -		47	13
Sol ad verticale - -		48	27

11ª.

Sol ad utrumque filum		52	4
Venus ad verticale -		52	37
Venus ad horizontale		53	42
Sol ad horizontale -		54	54
Sol ad verticale - -		56	15

12ª.

Sol ad utrumque filum	7	12	14
Venus ad verticale -		12	34
Venus ad horizontale		13	46
Sol ad horizontale -		15	2
Sol ad verticale - -		16	23

13ª.

Sol ad utrumque filum		18	45
Venus ad verticale -		19	1
Venus ad horizontale		20	14
Sol ad horizontale -		21	33
Sol ad verticale - -		22	53

Observatio 14ª.

	h	′	″
Sol ad utrumque filum	7	25	47
Venus ad verticale -		26	2
Venus ad horizontale		27	14
Sol ad horizontale -		28	32
Sol ad verticale - -		29	56

15ª.

Sol ad utrumque filum		33	56
Venus ad verticale -		-	-
Venus ad horizontale		35	20
Sol ad horizontale -		36	41
Sol ad verticale - -		38	9

16ª.

Sol ad utrumque filum		42	51
Venus ad verticale -		43	4
Venus ad horizontale		44	12
Sol ad horizontale -		45	37
Sol ad verticale - -		47	7

17ª.

Sol ad utrumque filum		49	34
Venus ad verticale -		49	47
Venus ad horizontale		50	56
Sol ad horizontale -		52	21
Sol ad verticale - -		53	52

18ª.

Sol ad utrumque filum		56	30
Venus ad verticale -		56	33
Venus ad horizontale		57	46
Sol ad horizontale -		59	12
Sol ad verticale - -	8	0	40

	h	′	″
Contactus interior - - - -	8	1	44
Contactus exterior - - - -		19	23

De contactu exteriore per tria aut quatuor ſecunda, de interiori verò vix dubitavi.

XLVII. *Ex-*

XLVII. *Extract from a Paper of Monſ.* De la Lande, *of the Royal Academy of Sciences at* Paris, *to Mr.* Gael Morris, *of the Tranſit of* Venus, *on the 6th* June 1761, *obſerved at* Tobolſk *in* Siberia, *by M.* Chappe.

Read Feb. 25, 1762.

	Apparent time. h	′	″
END of the eclipſe of the ſun, 2d June 1761, at - - - - - - - - - -	18	11	4
Internal contact of Venus with the ſun's limb at ingreſs, 5th June - - -	19	0	28
Internal contact at the egreſs, 6th June	0	49	20¼
External contact at the egreſs - - -	1	7	39⅖

Theſe obſervations were taken with a refracting teleſcope of 19 Paris feet focal length, with an eyeglaſs of 3 inches focus.

The leaſt diſtance of the ſouthern limb of Venus from the neareſt limb of the ſun, was meaſured by a micrometer fitted to a 10 feet teleſcope, and found to be = 6′ 2″, and the ſun's diameter was = 31′ 37″.

XLVIII. *Ob-*

XLVIII. *Obſervatio congreſſus* Veneris *cum* Sole, *habita* Lugduni Batavorum, *die* 6^a Junii 1761. *à* Joanne Lulofs, *in eadem Academ. Aſtronom. Matheſ. & Philoſoph. Profeſſore. Communicated by* Cha. Morton, *M. D. Secretary.*

Read March 4, 1762.

CUM diu expectatus atque merito deſideratus dies ſextus Junii hujus anni appropinquarat, ut celebratiſſimum Veneris tranſitum, accuratiori calculo, tum ſecundum tabulas ſolares Venereaſque Halleii, tum etiam ſecundum ſolares Abbatis De la Caille, Halleii verò Venereas à me ante duos ferme annos ſubjectum, legitima methodo, quæ ſincero aſtronomo ſatisfacere poſſet obſervarem, inde à fine menſis Martii omni virium contentione invigilavi directioni horologiorum, quibus obſervatorium noſtrum eſt inſtructum, eoque fine per repetitas obſervationes, ut vocantur, correſpondentium altitudinum Solis de novo examinavi teleſcopii meridionalis, à Siſſono, artifice Londinenſi, parati, ſitum; ut in re tam momentoſa omnem circumſpectionem adhiberem, ne forte è ſuo poſitu, quem per aliquot centenas obſervationes ante plures annos determinaveram, temporis tractu eſſet deturbatum. Poſtquam certiſſimus eram factus, omnia recte ſeſe habere (cum die quinto tranſitus Solis per medium teleſcopii filum vix $\frac{1}{4}$ unius minuti ſecundi differret à tempore, per ſat numeroſas obſervationes parum admodum à ſe invicem, quoad ipſum momentum tranſitus, diſſentientes, definito); poſtquam quartus et quintus Junii, ſine nube fuiſſent elapſi, veſpertino tempore diei quinti circa horam decimam cœlum Leidenſe undique et undique tegebatur, ita ut ſpes læta rarum hocce phænomenon obſervandi multum deminueretur.

Die ſexto obſervatorium conſcendens hora 3 30′ (in quo præcedenti die duos quadrantes; teleſcopium Newtonianum 7 pedum, ab Hearnio conſtructum, machinam parallacticam Caſſinianam, cui applicaveram teleſcopium 2 pedum, tubum dioptricum 8 pedum, in cujus foco poſitum erat filare reticulum 45 graduum, diſpoſueram) cœlum nubibus velatum deprehendebam. Verum circa horam 4 10′ per exiguum nubium interſtitium conſpicere mihi licebat Solem, Veneremque in ejus facie, inſtar maculæ nigricantis, ſic ſatis irregularis figuræ; ita ut margines (procul dubio ob refractionis viciſſitudines) quodammodo dentatæ apparerent: verum, cum vix per unicum temporis minutum phænomenon hocce præbebat ſeſe conſpiciendum, neque quadrantem, more Cl. De l'Iſle, neque teleſcopium Newtonianum, (cui adplicueram egregium micrometrum Bradleianum, ab Hearnio, juvante Siſſono, paratum) adhibere mihi licuit, ut verum Veneris ſitum detegerem.

Sæpe dein Venerem conſpexi per aliquot ſecunda, ad ſummum per unum aut ſeſqui-minutum: quater vel quinquies per nubes tenuiſſimas, abſque vitro colorato, aut fumo inquinato; tum mihi tum Sociis apparuit ut corona lucidiori cincta, quæ latitudinem habere videbatur $\frac{1}{6}$ vel ſaltem $\frac{1}{7}$ diametri Venereæ: attamen per vitra colorata, cum fulgentior quodammodo videbatur Sol, nihil prorſus hujus coronæ ſeſe monſtrabat; ita ut dubius hæream, an non hocce phænomenon fallaciæ opticæ totum ſit tribuendum.

Tandem hora 8 26′ 50″, tempore vero, obſervavi contactum interiorem, ſed per nubes tenuiores, ita ut vitrum fumo inquinatum, imo vitra cœrulea et viridia, (quæ ex præſcripto Cl. De l'Iſle ad manus erant, puriſſima) ſeponere debuerim: aſt duobus fere minutis antequam contactus exterior celebraretur, denſis nubibus tegebatur Solis facies; quæ cum tranſierant, nulla amplius Veneris apparebant veſtigia, ſed exacte circularis et nulla foveola deturpatus erat Solis limbus. Conſpexi autem contactum interiorem per teleſcopium Newtonianum ita diſpoſitum, ut nonageſies circiter diametrum objectorum augeret.

Id verò prorſus mirum exiſtit, Veneris circumferentiam etiam in contactu interiori, (cum Sol, ad altitudinem inſigniorem evectus, à refractionum viciſſitudinibus magis erat liberatus) quodammodo ſerratam apparuiſſe: quod ipſum in cauſa eſt, quod paucis aliquot ſecundis, tribus ſcilicet vel quatuor, ſerius contingere potuerit hicce contactus.

XLIX. *The Caſe of a Patient, who voided a large Stone through the Perinæum from the Urethra. Communicated to the Royal Society by Mr.* Joſeph Warner, *Surgeon, of* Guy's *Hoſpital, to whom this Memoir was addreſſed, for that Purpoſe, by Dr.* Frewen, *of* Rye *in* Suſſex.

Read Nov. 19, 1761.

HEnry Taught, of Haſtings in Suſſex, aged ſeventy-ſix, a ſtrong hail man, and naturally of a good conſtitution, was never ſubject to any nephritic or gravelly complaints for almoſt ſeventy years; but enjoyed, for the moſt part, a good ſhare of health, (though he had been expoſed, the greateſt part of his life-time, as a mariner, to the irregularities and inclemencies of that element, to which his occupation engaged him) till about ſix or ſeven years ago, when he had ſome gravelly complaints, and uneaſineſs in making water; which increaſed upon him progreſſively; and, for the laſt two years, he had ſo much pain in ſitting, that he was obliged to uſe a perforated chair, made for that purpoſe. But, for ſome months paſt, his increaſed pain would not permit him to ſit at all, even at his meals, which he uſed to take either ſtanding or lying. When he firſt came to be in this painful ſituation, there appeared a prominence on the right ſide of the perinæum, towards the hinder part of the ſcrotum; which, increaſing by degrees, felt hard and ſuperficial for ſome time; and the parts all about it grew ſo extremely ſore, and tender, that, at length, on the 24th of September laſt, upon his getting out of bed, a laceration thereof happened; and the ſtone, herewith ſhewn to this learned Society, was voided, falling down upon the floor.

Five days after this happened, I went to ſee the patient, in order to get a perfect knowledge of the circumſtances of the fact; the particulars of which I then communicated to my worthy friend Mr. Warner, ſurgeon, of Guy's hoſpital in London; who returned me a ſatisfactory account, from his own obſervations, of the manner by which a ſtone is contained in the urethra, &c. which I ſhall take the liberty of inſerting, after ſubmitting to the ſuperior judgment of this Society, a ſhort account of what I apprehended to be the original proceſs of nature, in the production of ſuch a phænomenon.

Dr. Boerhaave hath obſerved, from experiment, that if a quantity of recent urine be ſet, to digeſt in a tall glaſs, with a heat no greater than that of a healthy man's body, for the ſpace of three or four days, it will continually grow more and more red, fœtid, cadaverous, and alkaline, throwing off a ſtony matter to the ſides of the veſſel. From whence we learn, that calculous matter, by too long a detention of this excrementitious fluid in the bladder, may be eaſily generated; and a ſmall portion thereof, in its diſcharge from thence with the urine, may happen to be obſtructed in the paſſage of the urethra, ſo as to be incapable of getting either forward or backward, and thereby become the baſis of a ſtone; which, increaſing by the urinous ſupplies, may be accumulated

to

to as great a bulk as the part containing it will admit of.

Now, "the urethra, in caſes of this kind," according to the obſervation of my learned friend, "becomes a cyſt, which cyſt acquires a great degree "of hardneſs, and remains compact and whole, till "an inflammation is produced by its incapacity of "admitting any farther diſtenſion; which inflammation is ſoon after communicated to the integuments, by which means they become painful, "tender, and are eaſily lacerated." And this deſcription ſeems to correſpond exactly with what hath happened in the caſe of this old man.

Ever ſince the ſtone came away, this patient hath diſcharged no urine but by the wound; which, when I laſt ſaw him, was ſo much contracted, as to be no bigger than to admit into it a ſmall finger, and the parts were grown callous about it. I would have recommended him to proper care on that occaſion; but he would by no means hearken to me; ſeeming to be very happy in being freed from the cruel burden of the ſtone; and not regarding, I ſuppoſe, at his time of life, whether he could be helped in the diſcharge of his urine any other way.

Hatton-Garden, London, Nov. 12, 1761.

AS I am deſired by Dr. Frewen, in a letter to me, bearing date the 7th inſtant, to add whatever I ſhall think expedient to his memoir, I have, in conſequence of this requeſt, taken the liberty of obſerving, from a former letter of Dr. Frewen to me, bearing date the 17th of October laſt, that when this ſurprizingly large calculus was firſt voided, which was on the 24th of September 1761, it weighed ſix ounces and two drachms; that on the 29th of the ſame month it weighed ſix ounces, wanting one drachm and fifteen grains. On the 11th of October following, it weighed ſix ounces, wanting three drachms and one ſcruple. On the 17th of the ſame month, it weighed ſix ounces, wanting three drachms and half.

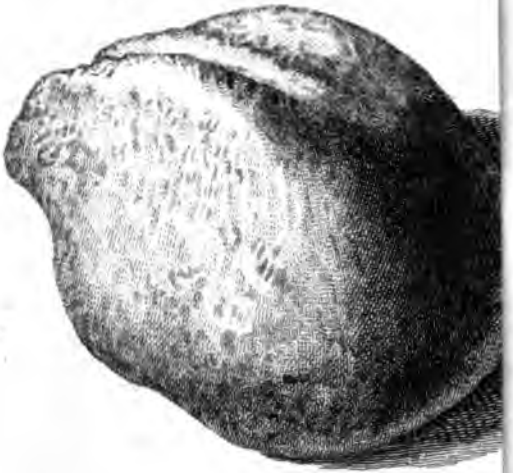
Give me leave to add farther to this paper, by obſerving, that, about March laſt, I produced two very remarkable calculi to the Royal Society, for their inſpection; when they did me the honour to deſire a written account of the caſe of the perſon, in whoſe urethra they were lodged. The whole of what I think worth troubling the Society with, upon this occaſion, is, that they had been for many years lodged in the urethra of one Robert Bolley, a young man, aged about twenty-two, and that they had produced no great inconvenience, or pain, till of late, when the integuments began to inflame; which inflammation commenced not long before he was put under my care. The conſequence of this change in the parts was extreme torture, a ſevere ſymptomatic fever, great waſting-away of the whole body, and almoſt a continual and involuntary diſcharge of ſmall quantities of urine.

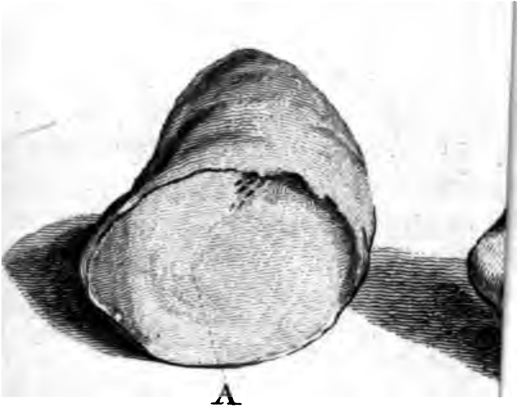
The miſerable object I have been juſt now deſcribing, was recommended to my aſſiſtance, by my ingenious friend and acquaintance Dr. Wollaſton, of Bury in Suffolk, in whoſe neighbourhood this patient lived, and from whence he was conveyed to London in a waggon.

The parts were then arrived to ſo great a degree of diſtention, inflammation, and tenderneſs, that, upon the journey, they burſt, and there was diſcharged thro' an opening made in the perinæum (that is, the ſpace betwixt the anus and ſcrotum) one of theſe ſtones; the other ſtone remained firmly fixed in the urethra, which I eaſily removed, having firſt cut away as much of the diſeaſed integuments of the acceleratores urinæ muſcles, and diſtended urethra, as I judged neceſſary to be removed for this purpoſe. After the removal of theſe parts, I brought together the lips of the wound, and kept them ſo, by means of that ſuture which ſurgeons call the twiſted ſuture, till the parts were united, which was effected in about a fortnight. Before the ſuture was applied, I introduced a ductile inſtrument, of a convenient ſize, through the penis into the bladder, by which means, the paſſage was kept equally diſtended.

This operation ſo effectually anſwered my expectation, as totally to remove the incontinence of urine, as well as every other ſymptom that had attended the complaint; and the patient was, in a ſhort time, reſtored to his uſual healthy ſtate and corpulency.

N. B. In the two inſtances I have juſt now related, as well as in the caſe of Thomas Bingham, whoſe hiſtory I communicated to this Society, on the 13th of December 1759, (vide Philoſophical Tranſactions for the year 1760.) I muſt obſerve, that theſe patients, according to the beſt information I could get, were never attacked with a ſuppreſſion of urine, or a regular fit of the ſtone; for which reaſons, I conclude, that the formation of theſe calculi did originally commence in the urethra itſelf, and that the ſtream of urine, in its courſe from the bladder through





A

the penis, had gradually formed thoſe grooves, or channels, ſo apparent on the ſurfaces of theſe compact and hard bodies, over which they occaſionally were voided; by this means, a paſſage for the urine always remained open and unobſtructed. [*Vide Tab.* IX. & X.]

Plate IX. repreſents the ſize, ſhape, and appearance, of the ſtone, in different attitudes, with the grooves on its ſuperior ſurface, that was voided through a laceration of the perinæum, as has been above deſcribed, in the caſe of Henry Taught, of Haſtings in Suſſex.

Plate X. repreſents the two ſtones that were lodged in the perinæum of Robert Bolley, a young man of twenty-two years of age, as has been already mentioned, with their poliſhed ſurfaces.

A and B, where they came in contact with each other.

In the ſame plate, are theſe two ſtones joined together, with their ſeveral eminences and depreſſions, and as they lay in contact with each other in the perinæum.

L. *An Account of the Caſe of a Boy, who had the Malleus of each Ear, and one of the Incus's, dropt out. Communicated by the Rev.* Philip Morant, *M. A. Rector of* St. Mary's *in* Colcheſter.

Read Nov. 19, 1761.

A Young lad, at Manningtree in Eſſex, " after about three or four weeks of a putrid, malignant, inflammatory fever, attended with a violent ſcarlet eruption on the ſkin, and ſwelling and ſoreneſs, and ſtuffage of the noſe, had the malleus of each ear, and one of the incus's, dropt out. Whether or no any of the reſt came away unobſerved, my friend cannot tell; but theſe were all he ſaw. Nor can he ſay, whether the membrane was deſtroyed, and diſcharged with the bones, or only ſo relaxed, as to give room for the bones to come without it; not having ſeen the bones, till after they were cleaned. But the conſequence is, his having almoſt abſolutely loſt his hearing; I ſay *almoſt*, becauſe, though he is quite deaf as to all common voices and ſounds, yet ſome violent and ſudden noiſes ſeem to affect him. But the organ of both ears ſeems to be ſo much deſtroyed, as to make it highly improbable, that he ſhould ever recover his hearing again. In all other reſpects, he is very well, and at preſent in good health. The coming away of thoſe bones ſeems the effect of an abſceſs, which affected the contents of the tympanum."

Another friend obſerves, that " his diſorder has been a malignant or ulcerous ſore throat, as he judges from the ſcarlet eruption; and the paſſage from the back of the fauces into the ear having lain open expoſed to its malign influence, an abſceſs has been formed in the tympanum, which has been deſtroyed; otherwiſe the bones could not come out at the other ear."

He had learned to read before this unhappy accident, and the people about him write down what they want to make him underſtand; at leaſt at preſent, till they have found out a readier method.

LI. *Obſervations concerning the Body of his late Majeſty,* October 26, 1760, *by* Frank Nicholls, *M. D. F. R. S. Phyſician to his late Majeſty.*

To the Right Honourable George *Earl of* Macclesfield,
Preſident of the Royal Society.

My Lord,

Read Nov. 26, 1761.

THE incloſed papers have been laid before the Lord Chamberlain, for his Majeſty's inſpection; and his Majeſty's anſwer was, That he ſaw no reaſon, why they may not be made public.

The burſting the ventricle of the heart is a caſe entirely unknown in phyſical writers; and muſt depend on many circumſtances, which rarely coincide.

I have uſed my beſt endeavours, to give a clear and ſatisfactory account of this very extraordinary affair; and I hope I have ſucceeded: but, if any thing abſtruſe ſhould appear, I truſt, it will be attributed rather to the nature of the caſe, than to any want of conſideration or reſpect for your Lordſhip, or the Society, in,

My Lord,

Your Lordſhip's

moſt obedient,

and moſt humble ſervant,

October 20, 1761.

Fran. Nicholls.

To the Right Honourable the Earl of Macclesfield, *Preſident of the Royal Society.*

My Lord,

THE circumſtances attending the death of the late King being ſuch, as are not (I apprehend) to be met with in any of the records of phyſical caſes, and ſuch, as, from the nature of the parts concerned, are not eaſily to be accounted for; I preſume it will be agreeable to your Lordſhip, to the Society in which you preſide, and to the learned world in general, if I lay before your Lordſhip, and the Society, a minute detail of what occurred on that remarkable and melancholy occaſion; with ſuch explanations, as ariſe from the circumſtances of the caſe.

According to the report of the pages then in waiting, about ſeven in the morning, Saturday, October 25th, a noiſe was ſomewhere heard, as if a large billet had tumbled down; and, upon enquiry, his

Majeſty was found fallen on the ground, ſpeechleſs and motionleſs, with a ſlight contuſed wound on his right temple. He appeared to have juſt come from his neceſſary-ſtool, and as if going to open his eſcritoir. Mr. Andrews (at that time ſurgeon to the houſhold) attempted to take away ſome blood; but in vain, as no ſigns of ſenſe, or motion, were obſerved, from the time of his fall.

The next day, (Sunday, October the 26th) by order of the Lord Chamberlain, I attended, with the two ſerjeant-ſurgeons, who were directed to open and embalm the Royal Body.

On opening the abdomen, all the parts therein contained were found in a natural and healthy ſtate, except that ſome hydatides (or watery bladders) were found between the ſubſtance of each kidney, and its internal coat. Theſe hydatides might, in time, have proved fatal, either by compreſſing and deſtroying the kidnies, ſo as to bring on an incurable ſuppreſſion of urine; or, by diſcharging a lymph into the cavity of the abdomen, might have formed a dropſy, not to be removed by any medicines: but, in the preſent caſe, theſe hydatides were of no conſequence, as none of them exceeded the bulk of a common walnut.

On opening the head, the brain was found in a healthy ſtate, no-ways loaded with blood, either in its proper veſſels, or in the contiguous ſinuſes of the dura mater.

Upon opening the cheſt, the lungs were in a natural ſtate, free from every appearance of inflammation, or tubercle: but upon examining the heart, its pericardium was found diſtended, with a quantity of coagulated blood, nearly ſufficient to fill a pint cup;

and, upon removing this blood, a round orifice appeared in the middle of the upper ſide of the right ventricle of the heart, large enough to admit the extremity of the little finger. Through this orifice, all the blood brought to the right ventricle had been diſcharged into the cavity of the pericardium; and, by that extravaſated blood, confined between the heart and pericardium, the whole heart was very ſoon neceſſarily ſo compreſſed, as to prevent any blood contained in the veins from being forced into the auricles; which, therefore, with the ventricles, were found abſolutely void of blood, either in a fluid or coagulated ſtate.

As, therefore, no blood could be tranſmitted through the heart, from the inſtant that the extravaſation was completed, ſo the heart could deliver none to the brain; and, in conſequence, all the animal and vital motions, as they depend on the circulation of the blood through the brain, muſt neceſſarily have been ſtopped, from the ſame inſtant; and his Majeſty muſt, therefore, have dropped down, and died inſtantaneouſly: And as the heart is inſenſible of acute and circumſcribed pain, his death muſt have been attended with as little of that diſtreſs, which uſually accompanies the ſeparation of the ſoul and body, as was poſſible, under any circumſtances whatſoever.

The above-mentioned appearances (as they ſhewed the immediate cauſe of his Majeſty's death) were thought ſufficient to form the report to his preſent Majeſty, and his Council. But as the very eminent and amiable character of his late Majeſty muſt make the nature of his death the object of every one's attention and inquiry; and as the caſe was exceedingly ſingular and extraordinary in itſelf; and as the heart muſt have been merely paſſive, and, conſequently, there muſt have been ſome other concurrent circumſtances neceſſary to produce ſuch an effect; I judged, at the time, when the report was drawn, that a more minute and exact detail would not only be expected by the world, but would be highly proper, as our inquiry furniſhed ſufficient matter.

Two queſtions naturally ariſe upon the face of our report; viz. by what means the right ſide of the heart became ſo charged with blood, as to be under a neceſſity of burſting? and how it could happen, that, as the ventricle (when under great diſtenſions) generally makes one continued cavity with the auricle, and is much thicker and ſtronger than the auricle, the blood ſhould, nevertheleſs, force its way, by burſting the ventricle, rather than the auricle, ſeemingly in contradiction to the known property of fluids, to force their way, where the reſiſtance is leaſt?

Upon examining the parts, we found the two great arteries, (the aorta and pulmonary artery, as far as they are contained within the pericardium) and the right ventricle of the heart ſtretched beyond their natural ſtate; and, in the trunk of the aorta, we found a tranſverſe fiſſure on its inner ſide, about an inch and half long, through which ſome blood had recently paſſed, under its external coat, and formed an elevated echymoſis. This appearance ſhewed the true ſtate of an incipient aneuriſm of the aorta; and confirmed the doctrine, which I had the honour to illuſtrate, by an experiment, to the ſatisfaction of the Society, in the Year 1728; [See the Philoſophical Tranſactions, N° 402.] viz. that the external coat of the artery may (and does) often controul an impetus of the blood, capable of burſting the internal (or ligamentous, coat; although this laſt is by much the thickeſt, and, ſeemingly, the ſtrongeſt.

In regard to this diſtention of the aorta; as his Majeſty had, for ſome years, complained of frequent diſtreſſes and ſinkings about the region of the heart; and as his pulſe was, of late years, obſerved to fall very much upon bleeding; it is not doubted, but that this diſtenſion of the aorta had been of long ſtanding, at leaſt to ſome degree; and, as the pulmonary artery was thereby neceſſarily compreſſed, and a reſiſtance, greater than natural, thereby oppoſed to the blood's diſcharge out of the right ventricle, it is reaſonable to conclude, that a diſtenſion and conſequent weakneſs of the pulmonary artery and right ventricle, to ſome degree, were nearly coeval with that of the aorta. But that the aorta had ſuffered a more extraordinary and violent diſtenſion, immediately antecedent to the burſting of the ventricle, is evident, from the recent fiſſure of the aorta, and the conſequent extravaſation of blood between its coats. Now, as this increaſed and violent diſtenſion of the aorta muſt have been attended with a proportionate preſſure upon the pulmonary artery, and, conſequently, an increaſed oppoſition to the paſſage of the blood out of the right ventricle; ſo that diſtenſion of the aorta muſt be conſidered, as the immediate cauſe of the right ventricle's being ſurcharged with blood, and conſequently of its burſting.

The immediate cauſe of this diſtenſion of the aorta, as likewiſe of its being determined to that particular time, are naturally explicable, from his Majeſty's having been at the neceſſary-ſtool; as the office then required cannot be executed, but by ſuch a preſſure on all the contents of the lower belly, and, conſequently, on the great deſcending artery, as muſt, of neceſſity, ſubject the trunk of the aorta, and all its upper branches, to a ſurcharge with blood continually increaſing, in proportion as the preſſure may happen to be continued longer, or exerted with greater violence, in conſequence of a coſtive habit, or any other reſiſtance.

As to the ſecond queſtion; viz. how it could happen, that the blood ſhould force its way rather through the ſide of the ventricle than of the auricle? ſince it is well known, that when the ventricle is fully diſtended with fluids, they will eaſily paſs back into the auricle; ſo that under ſuch a diſtenſion, as the ventricle muſt have ſuffered, before it burſt, it ſhould ſeem to have made one continued cavity with the auricle; of which cavity, the auricle, being by much the weakeſt part, muſt have been the moſt liable to a rupture. This certainly is the circumſtance, in which the very great ſingularity of the caſe before us conſiſts; and many difficulties offer againſt any obvious explanation.

Two circumſtances, however, ſeem to throw ſome light on this obſcure and difficult queſtion. The firſt conſiſts in the texture, connexions, and capacity, of the pericardium; the ſecond, in the order, in which the ſeveral ſurcharges muſt have ariſen.

The pericardium is a ſtrong tendinous membrane, inelaſtic in every direction, containing the two auricles, the two ventricles, and the two great arteries, as in a purſe: it is fixed to its contents at the back of the two auricles, where, by its connexion, it ſurrounds the two venæ cavæ: hence, paſſing along the arch formed by the aorta, it deſcends to the pulmonary artery, and continues round the orifices of the pulmonary veins, firmly attached to theſe ſeveral parts in its paſſage. By theſe connexions, theſe parts are all fixed in their ſeveral ſtations, incapable of ſeparating from each other, or ſhifting their ſituations, however they may happen to be compreſſed. The pericardium is generally ſaid to ſerve as a defence to the heart; but that defence ſeems to conſiſt chiefly, in preventing the right auricle from being ſtretched by the depreſſions (or complanations) of the diaphragm, in hunger and inſpiration, and, by its bearing firmly againſt the ſides of the auricles, to ſupport and ſtrengthen them againſt too great diſtenſions: for the cavity of the pericardium ſeems to be but little more, than commenſurate to the bulk of its contents, when one half of them are filled, and the other half empty. This will appear, upon endeavouring to fill the heart, with its auricles, and its two great arteries, with wax, at the ſame time, while it is incloſed in the pericardium; in which experiment, one or other of theſe cavities will be found to have been ſo compreſſed by the pericardium, as to have refuſed a free admittance to the wax, and will, therefore, be found proportionally empty.

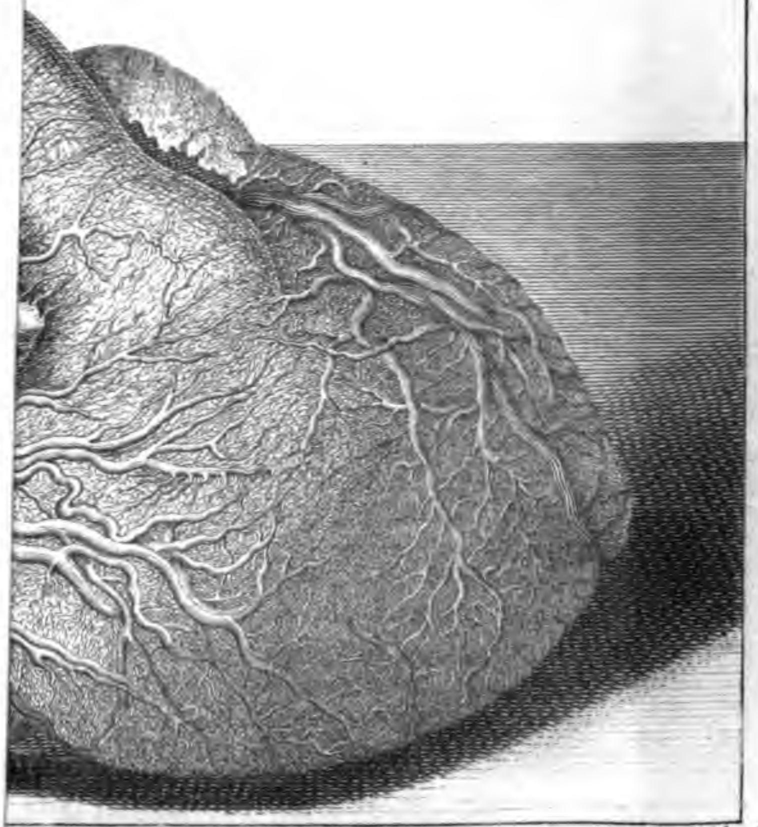
The inelaſtic texture, connexions, and capacity, of the pericardium, being thus ſtated, let us now conſider the order, in which the ſeveral diſtenſions muſt have ariſen, in the two great arteries and cavities of the heart, with the neceſſary effects of thoſe diſtenſions on the pericardium, and the parts which it contains.

The firſt diſtenſion (and this a great and violent one) muſt have ariſen in the aorta; and the conſequent preſſure on the pulmonary artery (by the aorta ſo diſtended) muſt have been ſufficient (either by degrees or at once) to ſtop the blood's diſcharge out of the right ventricle and pulmonary artery, and to diſtend both thoſe cavities greatly beyond their natural ſtate of repletion. So that, under theſe circumſtances, the two great arteries, and the right ventricle, muſt have been under an extraordinary and continued diſtenſion (and, conſequently, an increaſe of bulk) at the ſame time; whereas, in the natural ſtate of the body, theſe three cavities are alternately dilated and contracted, and the right ventricle is always proportionally diminiſhed in bulk, as the pulmonary artery is increaſed, and vice verſâ. So that, with reſpect to theſe three great cavities, (ſuppoſing that their ſeveral diſtenſions had been no greater than natural) the pericardium muſt have been obliged to contain one third more in proportion, than its capacity was formed to receive. During this time, the blood being ſtopped in its paſſage through the lungs, and its afflux to the left auricle and ventricle being thereby ſuſpended, the left auricle and ventricle muſt have remained in a contracted ſtate; in conſequence of which, the right ventricle had ample ſpace in the pericardium, to admit that degree of diſtenſion, which was previouſly requiſite for its burſting. But the right auricle (being fixed to its ſtation by its connections with the left auricle and the pericardium, and being firmly compreſſed againſt the pericardium, by the aorta, the pulmonary artery, and the right ventricle, all which appear to have been, at this time, greatly diſtended beyond their natural bulk) muſt have been thereby deprived of the ſpace in the pericardium, neceſſary to admit of its being diſtended; and the whole ſurcharge and diſtenſion muſt, by the preſſure of the pericardium on the auricle, neceſſarily have been confined to the right ventricle, till it burſt.

Had theſe ſurcharges ariſen in any other order, their effects muſt have been greatly different: as for inſtance, if the ſurcharge in the right ventricle had ariſen from any other preſſure, than from a diſtenſion of the aorta, the extraordinary bulk of the aorta, and its preſſure againſt the pulmonary artery, would not have exiſted, and the right auricle, not being then compreſſed againſt the pericardium, would have been at liberty to diſtend, till the blood had made its way through its ſides.

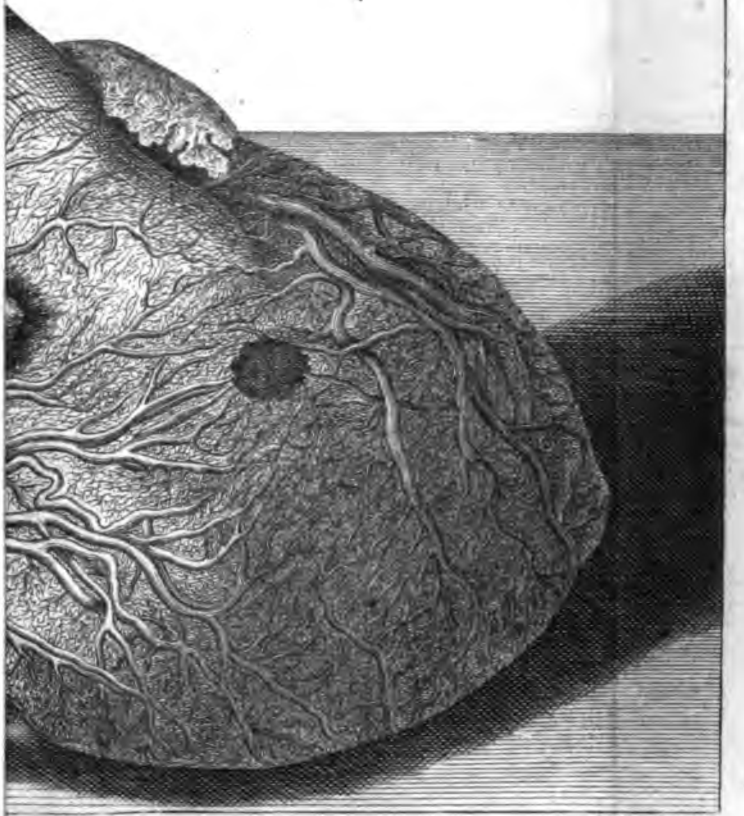
In confirmation of this power, here attributed to the pericardium, of ſtrengthening and ſupporting its contained parts, let it be obſerved, that, in the caſe under conſideration, the place of the fiſſure in the aorta is preciſely where the preſſure of the pericardium is kept off from the aorta, to a conſiderable degree, by the ſituation of the right auricle and the pulmonary artery.

My Lord, in order to give a clear and diſtinct idea of this very extraordinary caſe, I have here annexed two prints; [*Vide Tab.* XI. & XII.] the firſt of which ſhews the heart, as it appears when all its cavities and blood-veſſels are filled with wax; the other is the ſame print, having the orifice in the right ventricle,



J. Mynde sc.

Philos. Trans. Vol. LII. TAB. XII. p. 274.



J. Mynde sc.

ventricle, and the extravaſation covering the fiſſure in the aorta, exactly marked, as they appeared to,

My Lord,
Your Lordſhip's
moſt obedient
and moſt humble ſervant,

Frank Nicholls.

LII. *Of the Irregularities in the planetary Motions, cauſed by the mutual Attraction of the Planets: In a Letter to* Charles Morton, *M. D. Secretary to the Royal Society, by* Charles Walmeſley, *F. R. S. and Member of the Royal Academy of Sciences at* Berlin, *and of the Inſtitute at* Bologna.

SIR,

Read Dec. 10, 1761.

FInding that the influence, which the primary planets have upon one another, to diſturb mutually their motions, had been but little conſidered, I thought it a ſubject worthy of examination. The force of the ſun, to diſturb the moon's motion, flows from the general principle of *gravitation*, and has been fully aſcertained, both by theory and obſervation; and it follows, from the ſame principle, that all the planets muſt act upon one another, proportionally to the quantities of matter contained in their bulk, and inverſe ratio of the ſquares of their mutual diſtances; but as the quantity of matter contained in each of them, is but ſmall when compared to that of the ſun, ſo their action upon one another, is not ſo ſenſible as that of the ſun upon the moon. Aſtronomers generally contented themſelves with ſolely conſidering thoſe inequalities of the planetary motions, that ariſe from the elliptical figure of their orbits; but as they have been enabled, of late years, by the perfection of their inſtruments, to make obſervations with much more accuracy than before, they have diſcovered other variations, which they have not, indeed, been able yet to ſettle, but which ſeem to be owing to no other cauſe, but the mutual attraction of thoſe celeſtial bodies. In order, therefore, to aſſiſt the aſtronomers in diſtinguiſhing and fixing theſe variations, I ſhall endeavour to calculate their quantity, from the general law of gravitation, and reduce the reſult into tables, that may be conſulted, whenever obſervations are made.

I offer to you, at preſent, the firſt part of ſuch a theory, in which I have chiefly conſidered the effects produced by the actions of the earth and Venus upon each other. But the ſame propoſitions will likewiſe give, by proper ſubſtitutions, the effects of the other planets upon theſe two, or of theſe two upon the others. To obviate, in part, the difficulty of ſuch intricate calculations, I have ſuppoſed the orbits of the earth and Venus to be originally circular, and to ſuffer no other alteration, but what is occaſioned by their mutual attraction, and the attraction of the other planets. Where the forces of two planets are conſiderable, with reſpect to each other, as in the caſe of Jupiter and Saturn, it may be neceſſary, in ſuch computations, to have regard to the excentricity of their orbits; and this may be reſerved for a ſubject of future ſcrutiny. But the ſuppoſing the orbits of the earth and Venus to be circular, may, in the preſent caſe, be admitted, without difficulty, as the forces of theſe two planets are ſo ſmall, and the excentricity of their orbits not conſiderable. On theſe grounds, therefore, I have computed the variations, which are the effects of the earth's action: firſt, the variation of Venus's diſtance from the ſun; ſecondly, that of its place in the ecliptic; thirdly, the retrograde motion of Venus's nodes; and, fourthly, the variation of inclination of its orbit to the plane of the ecliptic.

The ſimilar irregularities in the motion of the earth, occaſioned by its gravitation to Venus, are here likewiſe computed: but it is to be obſerved, that the abſolute quantity of theſe irregularities is not here given, it being impoſſible, at preſent, to do it; becauſe the abſolute force of Venus is not known to us. I have, therefore, ſtated that planet's force by ſuppoſition, and have, accordingly, computed the effects it muſt produce; with the view, that the aſtronomers may compare their obſervations with the motions ſo calculated, and, from thence, diſcover how much the real force differs from that which has been ſuppoſed. But the exact determination of the force of Venus muſt be obtained, by obſervations made on the ſun's place, at ſuch times, when the effect of the other planets is either null or known.

The influence of Venus upon the earth being thus computed, that of the other planets upon the ſame, may likewiſe, hereafter, be conſidered: by which means, the different equations, that are to enter into the ſettling of the ſun's apparent place, will be determined; the change of the poſition of the plane of the earth's orbit will alſo be known; and, conſequently, the alteration that thence ariſes in the obliquity of the ecliptic, and in the longitude and latitude of the fixed ſtars. Theſe matters of ſpeculation are reſerved for another occaſion, in caſe what is here offered ſhould deſerve approbation.

I am glad to have it in my power to preſent you with this teſtimony of my gratitude for paſt favours, and of my reſpect for your diſtinguiſhed merit; and it is with ſincerity, I ſubſcribe myſelf,

SIR,

Your very humble ſervant,

Bath,
Nov. 21, 1761.

Cha. Walmeſley.

De Inæqualitatibus quas in motibus Planetarum generant ipſorum in ſe invicem actiones.

QUoniam in theoriæ hujus decurſu frequens erit uſus fluentium quæ arcubus circuli, vel eorum ſinibus, coſinibus, et ſinibus verſis, exprimuntur, idcircò lemma ſequens, quod alibi olim tradidi, lubet hîc apponere.

LEMMA.

LEMMA.

Dato coſinu arcûs cujuſvis, invenire coſinum et ſinum arcûs alterius qui ſit ad priorem in ratione λ ad 1.

Detur c coſinus arcûs A ad radium 1, et ſit arcus $B = \lambda A$, cujus coſinus dicatur t; eritque, ut notum eſt, $\dot{A} = \frac{-\dot{c}}{\sqrt{1-cc}}$, atque $\dot{B} = \lambda\dot{A} = \frac{-\dot{t}}{\sqrt{1-tt}}$. Ponatur $c = \frac{1+xx}{2x}$, et $t = \frac{1+yy}{2y}$, fietque $\dot{A} = \frac{\dot{x}}{x\sqrt{-1}}$, $\dot{B} = \frac{\dot{y}}{y\sqrt{-1}}$: ſed eſt $\dot{A} \,.\, \dot{B} :: 1 \,.\, \lambda$, adeoque $\frac{\lambda\dot{x}}{x} = \frac{\dot{y}}{y}$; unde log. $x^{\lambda} =$ log. y, et $x^{\lambda} = y$. Verùm æquationes $c = \frac{1+xx}{2x}$ et $t = \frac{1+yy}{2y}$ dant $x = c + \sqrt{cc-1}$, $x = c - \sqrt{cc-1}$, et $y = t + \sqrt{tt-1}$, $y = t - \sqrt{tt-1}$; unde eſt $x^{\lambda} = t \pm \sqrt{tt-1} = \overline{c \pm \sqrt{cc-1}}\,\big|^{\lambda}$, atque inde $2t = \overline{c + \sqrt{cc-1}}\,\big|^{\lambda} + \overline{c - \sqrt{cc-1}}\,\big|^{\lambda}$. Fiat igitur $c + \sqrt{cc-1} = l$, et $c - \sqrt{cc-1} = m$, eritque $lm = 1$, et $c =$ coſ. $A = \frac{l+m}{2}$, et ſin. $A = \frac{l-m}{2}\sqrt{-1}$; atque inde $t =$ coſ. $B = \frac{l^{\lambda}+m^{\lambda}}{2}$, et ſin. $B = \frac{l^{\lambda}-m^{\lambda}}{2}\sqrt{-1}$.

Itaque in circulo, cujus radius eſt 1, ſi duorum arcuum vel angulorum A et B alteruter B ſit ad alterum A ut numerus quilibet λ ad 1, et ponatur coſ. $A = \frac{l+m}{2}$, exiſtente $lm = 1$, erit ſin. A =

$= \frac{l-m}{2}\sqrt{-1}$, atque coſ. B $=$ coſs $\lambda A = \frac{l^{\lambda}+m^{\lambda}}{2}$, et ſin. B $=$ ſin. $\lambda A = \frac{l^{\lambda}-m^{\lambda}}{2}\sqrt{-1}$. Q. E. I.

COROLL. I.

Hinc habetur coſ. A $\times$ coſ. B $= \frac{l+m}{2} \times \frac{l^{\lambda}+m^{\lambda}}{2} = \frac{l^{\lambda+1}+m^{\lambda+1}}{4} + \frac{l^{\lambda-1}+m^{\lambda-1}}{4}$; ſed, quemadmodùm per hoc lemma eſt $\frac{l^{\lambda}+m^{\lambda}}{2} =$ coſ. λA, erit $\frac{l^{\lambda+1}+m^{\lambda+1}}{2} =$ coſ. $\overline{\lambda+1} \times A =$ coſ. $A + B$, atque $\frac{l^{\lambda-1}+m^{\lambda-1}}{2} =$ coſ. $\overline{\lambda-1} \times A =$ coſ. $\overline{B-A}$, adeoque coſ. A $\times$ coſ. B $= \frac{1}{2}$ coſ. $\overline{A+B} + \frac{1}{2}$ coſ. $\overline{B-A}$.

Atque hoc calculi methodo facilè eruuntur ſequentes formulæ pro duobus angulis A et B, advertendo eſſe coſ. $\overline{B-A} =$ coſ. $\overline{A-B}$, ſin. $\overline{B-A} = -$ ſin. $\overline{A-B}$, et coſ. $0 = 1$.

1°. Coſ. A $\times$ coſ. B $= \frac{1}{2}$ coſ. $\overline{A+B} + \frac{1}{2}$ coſ. $\overline{A-B}$.
2°. Sin. A $\times$ ſin. B $= -\frac{1}{2}$ coſ. $\overline{A+B} + \frac{1}{2}$ coſ. $\overline{A-B}$.
3°. Sin. A $\times$ coſ. B $= \frac{1}{2}$ ſin. $\overline{A+B} + \frac{1}{2}$ ſin. $\overline{A-B}$.
Atque ex illis hæ ſequentes eliciuntur,
4°. Coſ. $\overline{A+B} =$ coſ. A $\times$ coſ. B $-$ ſin. A $\times$ ſin. B.
5°. Coſ. $A - B =$ ſin. A $\times$ ſin. B $+$ coſ. A $\times$ coſ. B.
6°. Sin. $A + B =$ ſin. A $\times$ coſ. B $+$ coſ. A $\times$ ſin. B.
7°. Sin. $A - B =$ ſin. A $\times$ coſ. B $-$ coſ. A $\times$ ſin. B.

Tùm ex his valores tangentium haud ægrè derivantur.

Quippe cum ſit generatim pro quovis angulo A, $\text{tang. A} = \frac{\text{ſin. A}}{\text{coſ. A}}$, erit $\text{tang.}\ \overline{\text{A}+\text{B}} = \frac{\text{ſin.}\ \overline{\text{A}+\text{B}}}{\text{coſ.}\ \overline{\text{A}+\text{B}}} = \frac{\text{ſin. A} \times \text{coſ. B} + \text{coſ. A} \times \text{ſin. B}}{\text{coſ. A} \times \text{coſ. B} - \text{ſin. A} \times \text{ſin. B}} = \frac{\text{ſin. A} \times \text{coſ. B} + \text{coſ. A} \times \text{ſin. B}}{\text{coſ. A} \times \text{ſin. B.}}$ $\times \frac{\text{coſ. A} \times \text{ſin. B}}{\text{coſ. A} \times \text{coſ. B} - \text{ſin. A} \times \text{ſin. B}} = \overline{\frac{\text{tang. A}}{\text{tang. B}} + 1}$ $\times \frac{1}{\frac{1}{\text{tang. B}} - \text{tang. A}} = \frac{\text{tang. A} + \text{tang. B}}{1 - \text{tang. A} \times \text{tang. B}}$. Simili calculo prodit $\text{tang.}\ \overline{\text{A}-\text{B}} = \frac{\text{tang. A} - \text{tang. B}}{1 + \text{tang. A} \times \text{tang. B}}$. Unde ſtatui poſſunt,

1°. $\text{Tang.}\ \overline{\text{A}+\text{B}} = \frac{\text{tang. A} + \text{tang. B}}{1 - \text{tang. A} \times \text{tang. B}}$.

2°. $\text{Tang.}\ \overline{\text{A}-\text{B}} = \frac{\text{tang. A} - \text{tang. B}}{1 + \text{tang. A} \times \text{tang. B}}$.

3°. $\text{Tang. A} \times \text{tang. B} = \frac{\text{tang.}\ \overline{\text{A}+\text{B}} - \text{tang. A} - \text{tang. B}}{\text{tang. A+B}}$,

vel $\text{tang. A} \times \text{tang. B} = \frac{\text{tang. A} - \text{tang. B} - \text{tang.}\ \overline{\text{A}-\text{B}}}{\text{tang.}\ \overline{\text{A}-\text{B}}}$.

COROLL. II.

Erat in lemmate $\dot{\text{A}} = \frac{\dot{x}}{x\sqrt{-1}}$, unde eſt $\text{A}\sqrt{-1} = \text{log.}\ x$.

Denotet igitur E numerum cujus logarithmus hyperbolicus eſt 1, eritque $\text{E}^{\text{A}\sqrt{-1}} = x$, et cum ſit $x = c \pm \sqrt{cc - 1}$, inde obtinetur $c = \text{coſ. A} = \frac{\text{E}^{\text{A}\sqrt{-1}} + \text{E}^{-\text{A}\sqrt{-1}}}{2}$, atque $\text{ſin. A} = \frac{\text{E}^{\text{A}\sqrt{-1}} - \text{E}^{-\text{A}\sqrt{-1}}}{2\sqrt{-1}}$.

Sunt qui his ſinuum et coſinuum valoribus potiùs utuntur; verùm ii valores, quos exhibet corollarium præcedens, ſimpliciores ſunt et calculo plerumque aptiores.

COROLL. III.

Quoniam eſt $2 \times \text{coſ.}\, A = l + m$, erit

$$2^{\lambda} \times \overline{\text{coſ.}\, A}|^{\lambda} = \begin{cases} l^{\lambda} + \lambda l^{\lambda-1} m + \lambda \times \frac{\lambda - 1}{2} l^{\lambda-2} m^2 + \lambda \\ \quad \times \frac{\lambda - 1}{2} \times \frac{\lambda - 2}{3} l^{\lambda-3} m^3 +, \,\&c. \\ m^{\lambda} + \lambda m^{\lambda-1} l + \lambda \times \frac{\lambda - 1}{2} m^{\lambda-2} l^2 + \lambda \\ \quad \times \frac{\lambda - 1}{2} \times \frac{\lambda - 2}{3} m^{\lambda-3} l^3 +, \,\&c. \end{cases}$$

aſſumendo ſcilicet primos et ultimos terminos homologos ſeriei exprimentis quantitatem $\overline{l + m}|^{\lambda}$: unde, propter $lm = 1$, provenit

$$2^{\lambda-1} \times \overline{\text{coſ.}\, A}|^{\lambda} = \frac{l^{\lambda} + m^{\lambda}}{2} + \lambda \times \frac{l^{\lambda-2} + m^{\lambda-2}}{2} + \lambda \times \frac{\lambda - 1}{2} \times \frac{l^{\lambda-4} + m^{\lambda-4}}{2} + \lambda \times \frac{\lambda - 1}{2} \times \frac{\lambda - 2}{3} \times \frac{l^{\lambda-6} + m^{\lambda-6}}{2} +, \,\&c.$$

atque adeò per lemma

$$\overline{\text{coſ.}\, A}|^{\lambda} = \frac{1}{2^{\lambda-1}} \text{ in coſ.}\, \lambda A + \lambda \,\text{coſ.}\, \overline{\lambda - 2} \times A + \lambda \times \frac{\lambda - 1}{2} \text{coſ.}\, \overline{\lambda - 4} \times A + \lambda \times \frac{\lambda - 1}{2} \times \frac{\lambda - 2}{3} \text{coſ.}\, \overline{\lambda - 6} \times A +, \,\&c.$$

Ubi λ eſt numerus impar, terminus ultimus ſeriei erit ille in quo numerus λ, vel $\lambda - 2$, vel $\lambda - 4$, &c. qui multiplicat angulum A, evadit æqualis 1. Ubi verò λ eſt numerus par, terminus ultimus ſeriei erit ille in quo numerus prædictus evadit æqualis 0, quo in caſu ſemiſſis tantùm ultimi termini ſumenda eſt; cùm enim ſeries hæc colligatur ex numero pari terminorum homologorum, quæ tamen, ubi λ eſt numerus par, conſtare debet ex terminorum numero impari, ideò duplum exhibet terminum ultimum.

Simili modo cùm ſit $2 \times \text{ſin.}\, A = \overline{l - m} \times \sqrt{-1}$, erit

$$2^{\lambda} \times \overline{\text{ſin.}\, A}|^{\lambda} = \sqrt{-1} \times \left\{ \begin{array}{l} l^{\lambda} - \lambda l^{\lambda-1} m + \lambda \times \frac{\lambda-1}{2} l^{\lambda-2} m^{2} \\ \quad - \lambda \times \frac{\lambda-1}{2} \times \frac{\lambda-2}{3} l^{\lambda-3} m^{3} +, \\ \quad \&c. \\ \pm m^{\lambda} \mp \lambda m^{\lambda-1} l \pm \lambda \times \frac{\lambda-1}{2} m^{\lambda-2} l^{2} \\ \quad \mp \lambda \times \frac{\lambda-1}{2} \times \frac{\lambda-2}{3} m^{\lambda-3} l \pm, \\ \quad \&c. \end{array} \right.$$

Terminis inferioribus hujus ſeriei præfiguntur alternatim ſigna + — ubi λ eſt numerus par, et ſigna — + ubi λ eſt numerus impar, adeoque in priore caſu eſt

$$2^{\lambda-1} \times \overline{\text{ſin.}\, A}|^{\lambda} = \overline{\sqrt{-1}}|^{\lambda} \text{ in } \frac{l^{\lambda} + m^{\lambda}}{2} - \lambda \times \frac{l^{\lambda-2} + m^{\lambda-2}}{2} + \lambda \times \frac{\lambda-1}{2} \times \frac{l^{\lambda-4} + m^{\lambda-4}}{2} - \lambda \times \frac{\lambda-1}{2} \times \frac{\lambda-2}{3} \times \frac{l^{\lambda-6} + m^{\lambda-6}}{2} +, \&c.$$

et in caſu poſteriori

$$2^{\lambda-1} \times \overline{\text{ſin.}\, A}|^{\lambda} = \overline{\sqrt{-1}}|^{\lambda} \text{ in } \frac{l^{\lambda} - m^{\lambda}}{2} - \lambda \times \frac{l^{\lambda-2} - m^{\lambda-2}}{2} + \lambda \times \frac{\lambda-1}{2} \times \frac{l^{\lambda-4} - m^{\lambda-4}}{2} - \lambda \times \frac{\lambda-1}{2} \times \frac{\lambda-2}{3} \times \frac{l^{\lambda-6} - m^{\lambda-6}}{2} +, \&c.$$

Adeoque ſi λ ſit numerus par, erit

$$\overline{\text{ſin.}\, A}|^{\lambda} = \frac{1}{2^{\lambda-1}} \text{ in } \pm \text{coſ.}\, \lambda A \mp \lambda\, \text{coſ.}\, \overline{\lambda - 2} \times A \pm \lambda \times \frac{\lambda-1}{2}\, \text{coſ.}\, \overline{\lambda - 4} \times A \mp \lambda \times \frac{\lambda-1}{2} \times \frac{\lambda-2}{3}\, \text{coſ.}\, \overline{\lambda - 6} \times A \pm, \&c.$$

Signa hîc alternatim mutantur, et ſuperiora ſunt adhibenda, ubi λ exprimit unum ex numeris 4, 8, 12, 16, &c. quia tunc eſt $\overline{\sqrt{-1}}^{\,\lambda} = 1$; inferiora autem adhibenda, ubi λ exprimit unum ex numeris 2, 6, 10, 14, &c. quia tunc eſt $\overline{\sqrt{-1}}^{\,\lambda} = -1$.

Si λ ſit numerus impar, cùm per lemma ſit $\frac{l^{\lambda} - m^{\lambda}}{2}\sqrt{-1} = \text{ſin. } \lambda A$, et $\frac{l^{\lambda-2} - m^{\lambda-2}}{2}\sqrt{-1} = \text{ſin. } \overline{\lambda - 2} \times A$, &c. habetur

$$\overline{\text{ſin. } A}^{\,\lambda} = \frac{1}{2^{\lambda-1}} \text{ in } \pm \text{ſin. } \lambda A \mp \lambda \times \text{ſin. } \overline{\lambda - 2} \times A \pm \lambda \times \frac{\lambda - 1}{2} \text{ſin. } \overline{\lambda - 4} \times A \mp \lambda \times \frac{\lambda - 1}{2} \times \frac{\lambda - 2}{3} \text{ſin. } \overline{\lambda - 6} \times A \pm, \text{ \&c.}$$

ubi ſigna ſuperiora ſunt uſurpanda, cùm λ exprimit unum ex numeris 1, 5, 9, 13, &c. quia tunc eſt $\overline{\sqrt{-1}}^{\,\lambda} = \sqrt{-1}$; et ſigna inferiora, cùm λ fuerit unus ex numeris 3, 7, 11, 15, &c. quia tunc eſt $\overline{\sqrt{-1}}^{\,\lambda} = \sqrt{-1}$.

Notandum autem, ſeriei ultimum terminum eſſe illum in quo numerus λ, vel $\lambda - 2$, vel $\lambda - 4$, &c. eſt æqualis 1 ubi λ eſt numerus impar; atque terminum ultimum eſſe illum in quo prædictus numerus eſt æqualis 0 ubi λ eſt numerus par, quo in caſu ſemiſſis tantùm ultimi termini aſſumenda eſt ob rationem ſuperiùs datam.

Ex his ſinuum et coſinuum expreſſionibus alia hujuſmodi theoremata deducere liceret, ſed quæ hîc traduntur ad præſens inſtitutum ſufficiunt.

COROLL. IV.

Notum eſt fluentem fluxionis $\dot{A}$ coſ. A eſſe ſin. A, atque fluentem fluxionis $\dot{A}$ ſin. A eſſe ſin. verſ. A. Pariter ſi ſumatur arcus λA qui ſit ad arcum A ut numerus quilibet λ ad 1, cùm ſit $\lambda \dot{A}$ coſ. λA æqualis fluxioni ſinûs arcûs λA, erit flu. $\dot{A}$ coſ. $\lambda A = \frac{\text{ſin. } \lambda A}{\lambda}$, et flu. $\dot{A}$ ſin. $\lambda A = \frac{\text{ſin. verſ. } \lambda A}{\lambda}$. Itemque, ſi ad arcum λA adjungatur arcus datus d, cùm fluxio arcûs $\lambda A + d$ ſit æqualis $\lambda \dot{A}$, erit flu. $\dot{A}$ coſ. $\lambda A + d = \frac{\text{ſin. } \overline{\lambda A + d}}{\lambda}$, et flu. $\dot{A}$ ſin. $\overline{\lambda A + d} = \frac{\text{ſin. verſ. } \overline{\lambda A + d}}{\lambda}$.

Sumantur jam duo anguli, vel duo arcus λA et μA, qui ſint ad angulum, vel arcum A reſpectivè, ut λ et μ ad 1, atque per Coroll. II. habetur coſ. λA coſ. $\times \mu A = \frac{1}{2}$ coſ. $\overline{\lambda + \mu} \times A + \frac{1}{2}$ coſ. $\overline{\lambda - \mu} \times A$; unde erit fluens fluxionis $\dot{A}$ coſ. $\lambda A \times$ coſ. μA æqualis $\frac{\text{ſin. } \overline{\lambda + \mu} \times A}{2 \times \overline{\lambda + \mu}} + \frac{\text{ſin. } \overline{\lambda - \mu} \times A}{2 \times \overline{\lambda - \mu}}$.

Atque hoc methodo prodeunt ſequentes formulæ

1°. Flu. $\dot{A}$ coſ. $\lambda A \times$ coſ. $\mu A = \frac{\text{ſin. } \overline{\lambda + \mu} \times A}{2 \times \overline{\lambda + \mu}} + \frac{\text{ſin. } \overline{\lambda - \mu} \times A}{2 \times \overline{\lambda - \mu}}$.

2°. Flu. $\dot{A}$ ſin. $\lambda A \times$ ſin. $\mu A = - \frac{\text{ſin. } \overline{\lambda + \mu} \times A}{2 \times \overline{\lambda + \mu}} + \frac{\text{ſin. } \overline{\lambda - \mu} \times A}{2 \times \overline{\lambda - \mu}}$.

3°. Flu. $\dot{A}\ \text{ſin.}\ \lambda A \times \text{coſ.}\ \mu A = \frac{\text{ſin. verſ.}\ \overline{\lambda+\mu}\times A}{2\times\overline{\lambda+\mu}} + \frac{\text{ſin. verſ.}\ \overline{\lambda-\mu}\times A}{2\times\overline{\lambda-\mu}}$.

Advertendum autem eſt, ubi $\lambda = \mu$, tunc eſſe $\text{coſ.}\ \lambda A \times \text{coſ.}\ \mu A = \frac{1}{2}\ \text{coſ.}\ 2\lambda A + \frac{1}{2}$, $\text{ſin.}\ \lambda A \times \text{ſin.}\ \mu A = -\frac{1}{2}\ \text{coſ.}\ 2\lambda A + \frac{1}{2}$, $\text{ſin.}\ \lambda A \times \text{coſ.}\ \mu A = \frac{1}{2}\ \text{ſin.}\ 2\lambda A$; adeoque in hoc caſu formulæ præcedentes evadunt

1°. Flu. $\dot{A} \times \overline{\text{coſ.}\ \lambda A}^{\,2} = \frac{\text{ſin.}\ 2\lambda A}{4\lambda} + \frac{A}{2}$.

2°. Flu. $\dot{A} \times \overline{\text{ſin.}\ \lambda A}^{\,2} = -\frac{\text{ſin.}\ 2\lambda A}{4\lambda} + \frac{A}{2}$.

3°. Flu. $\dot{A} \times \text{ſin.}\ \lambda A \times \text{coſ.}\ \lambda A = \frac{\text{ſin. verſ.}\ 2\lambda A}{4\lambda}$.

Si angulo λA addatur angulus datus d, erit $\text{coſ.}\ \overline{\lambda A + d} \times \text{coſ.}\ \mu A = \frac{1}{2}\ \text{coſ.}\ \overline{\overline{\lambda+\mu}\times A + d} + \frac{1}{2}\ \text{coſ.}\ \overline{\overline{\lambda-\mu}\times A + d}$, atque inde

1°. Flu. $\dot{A}\ \text{coſ.}\ \overline{\lambda A + d} \times \text{coſ.}\ \mu A = \frac{\text{ſin.}\ \overline{\overline{\lambda+\mu}\times A+d}}{2\times\overline{\lambda+\mu}} + \frac{\text{ſin.}\ \overline{\overline{\lambda-\mu}\times A+d}}{2\times\overline{\lambda-\mu}}$.

2°. Flu. $\dot{A}\ \text{ſin.}\ \overline{\lambda A + d} \times \text{ſin.}\ \mu A = -\frac{\text{ſin.}\ \overline{\overline{\lambda+\mu}\times A+d}}{2\times\overline{\lambda+\mu}} + \frac{\text{ſin.}\ \overline{\overline{\lambda-\mu}\times A+d}}{2\times\overline{\lambda-\mu}}$.

3°. Flu. $\dot{A}\ \text{ſin.}\ \overline{\lambda A + d} \times \text{coſ.}\ \mu A = \frac{\text{ſin. verſ.}\ \overline{\overline{\lambda+\mu}\times A+d}}{2\times\overline{\lambda+\mu}} + \frac{\text{ſin. verſ.}\ \overline{\overline{\lambda-\mu}\times A+d}}{2\times\overline{\lambda-\mu}}$.

4°. Flu. $\dot{A}\ \text{coſ.}\,\overline{\lambda A+d}\times\text{ſin.}\,\mu A=\frac{\text{ſin. verſ.}\,\overline{\overline{\lambda+\mu}\times A+d}}{2\times\overline{\lambda+\mu}}$ $-\frac{\text{ſin. verſ.}\,\overline{\overline{\lambda-\mu}\times A+d}}{2\times\overline{\lambda-\mu}}$.

Si fuerit $\lambda=\mu$, erit $\text{coſ.}\,\overline{\lambda A+d}\times\text{coſ.}\,\lambda A=$ $\frac{1}{2}\,\text{coſ.}\,\overline{2\lambda A+d}+\frac{1}{2}\,\text{coſ.}\,d$, &c. adeoque formulæ præcedentes in has abeunt,

1°. Flu. $\dot{A}\ \text{coſ.}\,\overline{\lambda A+d}\times\text{coſ.}\,\lambda A=\frac{\text{ſin.}\,\overline{2\lambda A+d}}{4\lambda}$ $+\frac{\text{coſ.}\,d}{2}A$.

2°. Flu. $\dot{A}\ \text{ſin.}\,\overline{\lambda A+d}\times\text{ſin.}\,\lambda A=-\frac{\text{ſin.}\,\overline{2\lambda A+d}}{4\lambda}$ $+\frac{\text{coſ.}\,d}{2}A$.

3°. Flu. $\dot{A}\ \text{ſin.}\,\overline{\lambda A+d}\times\text{coſ.}\,\lambda A=\frac{\text{ſin. verſ.}\,\overline{2\lambda A+d}}{4\lambda}$ $+\frac{\text{ſin.}\,d}{2}A$.

4°. Flu. $\dot{A}\ \text{coſ.}\,\overline{\lambda A+d}\times\text{ſin.}\,\lambda A=\frac{\text{ſin. verſ.}\,\overline{2\lambda A+d}}{4\lambda}$ $-\frac{\text{ſin.}\,d}{2}A$.

PROPOSITIO I. PROBLEMA.

In ſyſtemate duorum planetarum circa Solem in orbibus penè circularibus revolventium, requiratur vis planetæ exterioris ad perturbandum motum interioris.

Revolvantur planetæ duo P et Q (Fig. 1.) in eodem plano circa Solem in S, et jungantur SP, SQ, PQ.

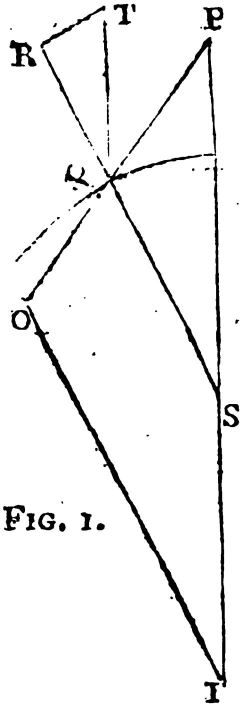


FIG. 1.

Orbis planetæ interioris Q, cujus motus hîc inveſtigamus, circularis ſupponitur niſi quatenùs mutatur ejus figura vi planetæ P; orbem verò planetæ P ut accuratè circularem habemus. Poſitâ ergò unitate pro diſtantiâ corporis Q à Sole ubi ambo planetæ verſantur in conjunctione cum Sole, fiant $SQ = x$, $PQ = z$, $SP = k$; tumque vis attractionis Solis in diſtantiâ æquali 1 ſit ad vim attractionis planetæ P in eâdem diſtantiâ ut 1 ad φ, eritque $\frac{\varphi}{z^2}$ gravitas planetæ Q in planetam P. Producatur jam, ſi opus eſt, PQ ad O ut ſit $PO = \frac{\varphi}{z^2}$, et ductâ OI parallelâ ipſi QS occurrente rectæ PS productæ in I, propter triangula ſimilia PQS, POI, erit $PQ \cdot PS :: PO \cdot PI$, hoc eſt, $PI = \frac{\varphi k}{z^3}$, atque $PQ \cdot QS :: PO \cdot OI$, hoc eſt, $OI = \frac{\varphi x}{z^3}$. Sed, quia parùm differt x ab unitate et admodùm exigua eſt vis φ, pro x ſcribi poteſt 1 in omnibus iis terminis qui ducuntur in φ, adeoque $OI = \frac{\varphi}{z^3}$. Ex vi PI auferatur vis $\frac{\varphi}{k^2}$ qua gravitat Sol in planetam P, et vis reſidua $\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}$ eſt ea qua perturbatur motus planetæ Q in directione parallelâ rectæ PS: nàm cùm motus planetarum referantur ad Solem ſpectatum tanquàm immotum, vis $\frac{\varphi k}{z^3}$ pars ea $\frac{\varphi}{k^2}$, qua ſimul urgentur Sol et planeta Q verſus P ſecundum lineas parallelas, non mutat corporum S et Q ſitum ad ſe invicem, ideoque differentia virium ſola perturbationem inducit.

Quare differentia illa, nimirùm $\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}$, exponatur per lineam QT parallelam rectæ PS, et in SQ demiſſo perpendiculo TR, vis QT reſolvetur in vires TR, QR, eritque vis QT ad vim TR ut radius 1 ad ſinum anguli QSP, adeoque vis TR $= \overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ ſin. QSP, et vis QR $= \overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ coſ. QSP. Ex vi autem QR tollatur vis OI utpotè in contrarium agens, et manebit vis $\overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ coſ. QSP $- \frac{\varphi}{z^3}$. Vires igitur, quibus planeta P perturbat motum planetæ Q quatenùs in eodem plano moventur, ſunt

1°. Vis TR ad radium QS perpendicularis, qua augetur vel minuitur area tempore dato deſcripta, eſtque æqualis $\overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ ſin. QSP.

2°. Vis $\frac{\varphi}{z^3} \times \overline{k \text{ coſ. } QSP - 1} - \frac{\varphi}{k^2}$ coſ. QSP, qua retrahitur planeta Q à Sole in directione radii SQ.

Ut autem harumce virium expreſſiones formam induant calculo accommodam, ope trianguli PSQ habebitur $\overline{PQ}^2 = zz = kk + xx - 2kx \times$ coſ. QSP, ſive, poſitâ $x = 1$ ob rationem dictam, $zz = kk$

+ 1

$+ 1 - 2k \times \text{coſ.}\ QSP$. Aſſumatur jam angulus s qui ſemper ſit ad angulum QSP in ratione n ad 1, eritque $QSP = \frac{1}{n}s$, et poſito $kk + 1 = tt$, et $\frac{2k}{t^2} = b$, erit $z^2 = t^2 \times \overline{1 - b\ \text{coſ.}\ \frac{1}{n}s}$, hincque $\frac{1}{z^3} = \frac{1}{t^3} \times \overline{1 - b\ \text{coſ.}\ \frac{1}{n}s}^{\,-\frac{3}{2}}$. Si b fuerit unitati ferè æqualis, et evolvatur quantitas $\overline{1 - b\ \text{coſ.}\ \frac{1}{n}s}^{\,-\frac{3}{2}}$ in ſeriem modo ſolito, ſeries illa parùm convergit, eſtque ad operationes analyticas minùs commoda. Series igitur alia inveſtiganda eſt, et quia ex lemmate patet hujuſmodi quantitatem $\overline{\text{coſ.}\ A}^{\,n}$ exprimi poſſe aggregato terminorum, quorum ſinguli ducuntur in coſinus angulorum qui ſunt anguli A multiplices, generatim ſupponemus $\overline{1 - b\ \text{coſ.}\ \frac{1}{n}s}^{\,m} = R + S\ \text{coſ.}\ \frac{1}{n}s + T\ \text{coſ.}\ \frac{2}{n}s + V\ \text{coſ.}\ \frac{3}{n}s + W\ \text{coſ.}\ \frac{4}{n}s +$, &c.

Atque ut inveniantur valores coefficientium R, S, T, &c. ſumatur utrinque fluxio, nempe $\frac{mb}{n}\dot{s} \times \text{ſin.}\ \frac{1}{n}s \times \overline{1 - b\ \text{coſ.}\ \frac{1}{n}s}^{\,m-1} = - S \times \frac{1}{n}\dot{s} \times \text{ſin.}\ \frac{1}{n}s - T \times \frac{2}{n}\dot{s} \times \text{ſin.}\ \frac{2}{n}s - V \times \frac{3}{n}\dot{s} \times \text{ſin.}\ \frac{3}{n}s - W \times \frac{4}{n}\dot{s} \times \text{ſin.}\ \frac{4}{n}s -$, &c. atque ducatur æquatio hæc in $\overline{1 - b\ \text{coſ.}\ \frac{1}{n}s}$, et ſubſtituto pro $\overline{1 - b\ \text{coſ.}\ \frac{1}{n}s}^{\,m}$ ipſius valore $R + S\ \text{coſ.}\ \frac{1}{n}s + T\ \text{coſ.}\ \frac{2}{n}s +$, &c. fiet $mb \times \text{ſin.}\ \frac{1}{n}s$

×

$$\overline{\times R + S\,\text{coſ.}\,\tfrac{1}{n}s + T\,\text{coſ.}\,\tfrac{2}{n}s + V\,\text{coſ.}\,\tfrac{3}{n}s +, \&c.} = \overline{1 - b\,\text{coſ.}\,\tfrac{1}{n}s}$$

$$\overline{\times - S \times \text{ſin.}\,\tfrac{1}{n}s + 2T \times \text{ſin.}\,\tfrac{2}{n}s \times 3V \times \text{ſin.}\,\tfrac{3}{n}s + W \times \text{ſin.}\,\tfrac{4}{n}s +, \&c.}$$

et factâ multiplicatione, cùm ſit (per Coroll. I. Lem.) $\text{ſin.}\,\frac{1}{n}s \times \text{coſ.}\,\frac{r}{n}s = \frac{1}{2}\,\text{ſin.}\,\frac{r+1}{n}s - \frac{1}{2}\,\text{ſin.}\,\frac{r-1}{n}s$, ac $\text{ſin.}\,\frac{r}{n}s \times \text{coſ.}\,\frac{1}{n}s = \frac{1}{2}\,\text{ſin.}\,\frac{r+1}{n}s + \frac{1}{2}\,\text{ſin.}\,\frac{r-1}{n}s$, emerget

$$\left.\begin{array}{l} +2mbR \\ +2S \\ -2bT \\ -mbT \end{array}\right\} \times \text{ſin.}\,\tfrac{1}{n}s \left\{\begin{array}{l} +mbS \\ -bS \\ +4T \\ -3bV \\ -mbV \end{array}\right\} \times \text{ſin.}\,\tfrac{2}{n}s \left\{\begin{array}{l} +mbT \\ -2bT \\ +6V \\ -4bW \\ -mbW \end{array}\right\} \times \text{ſin.}\,\tfrac{3}{n}s, \&c. = 0$$

Deinde nihilo æquando ſingulos terminos, prodeunt $T = \frac{2S + 2mbR}{\overline{m+2} \times b}$, $V = \frac{4T + \overline{m-1} \times bS}{\overline{m+3} \times b}$, $W = \frac{6V + \overline{m-2} \times bT}{\overline{m+4} \times b}$, &c. quorum valorum progreſſus ſatis manifeſtus eſt.

Datis igitur primis duobus coefficientibus R et S, dabuntur et reliqui: R et S autem ſic inveniuntur.

Eſt $\overline{1 - b\,\text{coſ.}\,\frac{1}{n}s}\big|^{m} = 1 - mb\,\text{coſ.}\,\frac{1}{n}s + m \times \frac{m-1}{2} b^2\, \overline{\text{coſ.}\,\frac{1}{n}s}\big|^{2} - m \times \frac{m-1}{2} \times \frac{m-2}{3} b^3\, \overline{\text{coſ.}\,\frac{1}{n}s}\big|^{3}$, &c. $= R + S\,\text{coſ.}\,\frac{1}{n}s + T\,\text{coſ.}\,\frac{2}{n}s + V\,\text{coſ.}\,\frac{3}{n}s +$, &c. Evolvantur termini $\overline{\text{coſ.}\,\frac{1}{n}s}\big|^{2}$, $\overline{\text{coſ.}\,\frac{1}{n}s}\big|^{4}$, $\overline{\text{coſ.}\,\frac{1}{n}s}\big|^{6}$, &c. per methodum traditam in Coroll. III. Lem. ac, collectis ſimul omnibus terminis qui nullo coſinu afficiuntur, prodibit

$$R = 1 + \frac{m}{2} \times \frac{m-1}{2} b^2 + \frac{m}{2} \times \frac{m-1}{2} \times \frac{m-2}{4} \times \frac{m-3}{4} b^4 + \frac{m}{2} \times \frac{m-1}{2} \times \frac{m-2}{4} \times \frac{m-3}{4} \times \frac{m-4}{6} \times \frac{m-5}{6} b^6 +, \text{\&c.}$$

cujus ſeriei progreſſio ſatis patet; atque adeò, cum ſit in hoc noſtro problemate $m = -\frac{3}{2}$, erit

$$R = 1 + \frac{3 \times 5}{4 \times 4} b^2 + \frac{3 \times 5}{4 \times 4} \times \frac{7 \times 9}{8 \times 8} b^4 + \frac{3 \times 5}{4 \times 4} \times \frac{7 \times 9}{8 \times 8} \times \frac{11 \times 13}{12 \times 12} b^6 + \frac{3 \times 5}{4 \times 4} \times \frac{7 \times 9}{8 \times 8} \times \frac{11 \times 13}{12 \times 12} \times \frac{15 \times 17}{16 \times 16} b^8 +, \text{\&c.}$$

Inſpicienti indolem hujus ſeriei patebit terminum quemlibet æquari termino antecedenti ducto in $\frac{\overline{r+1} \times \overline{r-1}}{r^2} b^2$, ſive $\frac{r^2-1}{r^2} b^2$, r exiſtente æquali numero quadruplicato terminorum præcedentium: ſic terminus ſextus, quia habetur in hoc caſu $r = 5 \times 4 = 20$, æqualis eſt termino quinto $\frac{3 \times 5}{4 \times 4} \ldots\ldots \frac{15 \times 17}{16 \times 16} b^8$ ducto in $\frac{19 \times 21}{20 \times 20} b^2$.

Termino igitur quovis hujus ſeriei dicto B; terminus ſubſequens erit $Bb^2 \times \frac{r^2-1}{r^2}$: et manente deinceps eodem, quem in hoc termino habet, numeri r valore, termini ſubſequentes erunt, $Bb^4 \times \frac{r^2-1}{r^2} \times \frac{\overline{r+4}|^2-1}{\overline{r+4}|^2}$, $Bb^6 \times \frac{r^2-1}{r^2} \times \frac{\overline{r+4}|^2-1}{\overline{r+4}|^2} \times \frac{\overline{r+8}|^2-1}{\overline{r+8}|^2}$, $Bb^8 \times \frac{r^2-1}{r^2} \ldots\ldots \frac{\overline{r+12}|^2-1}{\overline{r+12}|^2}$, &c. Sed eſt $\frac{r^2-1}{r^2} = 1 - \frac{1}{r^2}$, $\frac{\overline{r+4}|^2-1}{\overline{r+4}|^2} = 1 - \frac{1}{\overline{r+4}|^2}$, &c. et ſi fuerit r

numerus aliquantùm magnus, erit $\frac{r^2-1}{r^2}\times\frac{\overline{r+4}|^2-1}{\overline{r+4}|^2}$ $=1-\frac{1}{r^2}-\frac{1}{\overline{r+4}|^2}$, et $\frac{r^2-1}{r^2}\times\frac{\overline{r+4}|^2-1}{\overline{r+4}|^2}\times\frac{\overline{r+8}|^2-1}{\overline{r+8}|^2}$ $=1-\frac{1}{r^2}-\frac{1}{\overline{r+4}|^2}-\frac{1}{\overline{r+8}|^2}$, atque ita porrò, rejiciendo fractiones hujus generis $\frac{1}{r^2\times\overline{r+4}|^2}$ et alias his minores.

Unde termini omnes prædicti, incipiendo à termino B, erunt

$$\begin{array}{rcl}
B+Bb^2+Bb^4+Bb^6+Bb^8+,\ \&c. & = & B\times\frac{1}{1-b^2}\\
-\frac{Bb^2}{r^2}-\frac{Bb^4}{r^2}-\frac{Bb^6}{r^2}-\frac{Bb^8}{r^2}-,\ \&c. & = & -\frac{B}{r^2}\times\frac{b^2}{1-b^2}\\
-\frac{Bb^4}{\overline{r+4}|^2}-\frac{Bb^6}{\overline{r+4}|^2}-\frac{Bb^8}{\overline{r+4}|^2}-,\ \&c. & = & -\frac{B}{\overline{r+4}|^2}\times\frac{b^4}{1-b^2}\\
-\frac{Bb^6}{\overline{r+8}|^2}-\frac{Bb^8}{\overline{r+8}|^2}-,\ \&c. & = & -\frac{B}{\overline{r+8}|^2}\times\frac{b^6}{1-b^2}\\
-\frac{Bb^8}{\overline{r+12}|^2}-,\ \&c. & = & -\frac{B}{\overline{r+12}|^2}\times\frac{b^8}{1-b^2}\\
\&c. & & \&c.
\end{array}$$

ac proinde tandem fit

$$R=1+\frac{3\times5}{4\times4}b^2+\frac{3\times5}{4\times4}\times\frac{7\times9}{8\times8}b^4+\frac{3\times5}{4\times4}\times\frac{7\times9}{8\times8}\times\frac{11\times13}{12\times12}b^6\times\frac{3\times5}{4\times4}\cdots\frac{15\times17}{16\times16}b^8+,\ \&c.+\frac{B}{1-b^2}\times\overline{1-\frac{b^2}{r^2}-\frac{b^4}{\overline{r+4}|^2}-\frac{b^6}{\overline{r+8}|^2}-\frac{b^8}{\overline{r+12}|^2}-\frac{b^{10}}{\overline{r+16}|^2}-,\ \&c.}$$

Unde ſi, computatis, exempli gratiâ, decem terminis, undecimus deſignetur per B, erit $r=10\times4=40$, et ſumma illorum decem terminorum addita ſummæ ſeriei $\frac{B}{1-b^2}\times\overline{1-\frac{b^2}{r^2}-\frac{b^4}{\overline{r\times 4}|^2}-}$, &c. dabit valorem ipſius R.

Simili modo ſi in æquatione prædictâ $1-mb\,\text{coſ.}\,\frac{1}{n}s+m\times\frac{m-1}{2}b^2\times\overline{\text{coſ.}\,\frac{1}{n}s}|^2+m\times\frac{m-1}{2}\times\frac{m-2}{3}b^3\times\overline{\text{coſ.}\,\frac{1}{n}s}|^3+$, &c. $=R+S\,\text{coſ.}\,\frac{1}{n}s+T\,\text{coſ.}\,\frac{2}{n}s+V\,\text{coſ.}\,\frac{3}{n}s+$, &c. evolvantur quantitates $\overline{\text{coſ.}\,\frac{1}{n}s}|^3$, $\overline{\text{coſ.}\,\frac{1}{n}s}|^5$, $\overline{\text{coſ.}\,\frac{1}{n}s}|^7$, &c. in ſuos valores, prout in Coroll. III. Lem. edoctum eſt, et colligantur omnes termini qui ducuntur in coſ. $\frac{1}{n}s$ exurget

$$S=-mb-m\times\frac{m-1}{2}\times\frac{m-2}{4}b^3-m\times\frac{m-1}{2}\times\frac{m-2}{4}\times\frac{m-3}{4}\times\frac{m-4}{6}b^5-m\times\frac{m-1}{2}\times\frac{m-2}{4}\times\frac{m-3}{4}\times\frac{m-4}{6}\times\frac{m-5}{6}\times\frac{m-6}{8}b^7-\text{, \&c.}$$

ſive, poſito $m=-\frac{3}{2}$,

$$S=\frac{3}{2}b+\frac{3}{2}\times\frac{5\times 7}{4\times 8}b^3+\frac{3}{2}\times\frac{5\times 7}{4\times 8}\times\frac{9\times 11}{8\times 12}b^5+\frac{3}{2}\times\frac{5\times 7}{4\times 8}\times\frac{9\times 11}{8\times 12}\times\frac{13\times 15}{12\times 16}b^7+\frac{3}{2}\cdots\cdots\frac{13\times 15}{12\times 16}\times\frac{17\times 19}{16\times 20}b^9+\text{, \&c.}$$

Patet autem terminum quemlibet hujus ſeriei æquari termino antecedenti ducto in $\frac{\overline{r+1}\times\overline{r+3}}{r\times\overline{r+4}}b^2$, exiſtente r æquali numero terminorum præcedentium quadruplicato: ſic terminus ſextus, quia tunc $r=5\times 4=20$, eſt æqualis termino quinto $\frac{1}{2}\cdots\cdots\frac{17\times19}{16\times20}b^{8}$ ducto in $\frac{21\times23}{20\times24}b^{2}$. Quamobrem termino quovis hujus ſeriei dicto B, terminus ſubſequens erit $Bb^{2}\times\frac{\overline{r+1}\times\overline{r+3}}{r\times\overline{r+4}}$, ſive $Bb^{2}\times\overline{1+\frac{3}{r\times\overline{r+4}}}$, et manente jam eodem valore numeri r, termini reliqui erunt, $Bb^{4}\times\overline{1+\frac{3}{r\times\overline{r+4}}}\times\overline{1+\frac{3}{\overline{r+4}\times\overline{r+8}}}$, $Bb^{6}\times\overline{1+\frac{3}{r\times\overline{r+4}}}\times\overline{1+\frac{3}{\overline{r+4}\times\overline{r+8}}}\times\overline{1+\frac{3}{\overline{r+8}\times\overline{r+12}}}$, &c. Sed ſi fuerit r numerus aliquantùm magnus, erit $\overline{1+\frac{3}{r\times\overline{r+4}}}\times\overline{1+\frac{3}{\overline{r+4}\times\overline{r+8}}}=1+\frac{3}{r\times\overline{r+4}}+\frac{3}{\overline{r+4}\times\overline{r+8}}$ quamproximè, et $\overline{1+\frac{3}{r\times\overline{r+4}}}\times\overline{1+\frac{3}{\overline{r+4}\times\overline{r+8}}}\times\overline{1+\frac{3}{\overline{r+8}\times\overline{r+12}}}=1+\frac{3}{r\times\overline{r+4}}+\frac{3}{\overline{r+4}\times\overline{r+8}}+\frac{3}{\overline{r+8}\times\overline{r+12}}$, &c. Unde termini omnes prædicti incipientes à termino B erunt

$$
\begin{array}{llllll}
B+Bb^{2}+Bb^{4}+Bb^{6}+Bb^{8} & +,\&c. = \frac{B}{1-b^{2}} \\
+\frac{3Bb^{2}}{r\times\overline{r+4}}+\frac{3Bb^{4}}{r\times\overline{r+4}}+\frac{3Bb^{6}}{r\times\overline{r+4}}+\frac{3Bb^{8}}{r\times\overline{r+4}} & +,\&c. = \frac{3B}{r\times\overline{r+4}}\times\frac{b^{2}}{1-b^{2}} \\
+\frac{3Bb^{4}}{\overline{r+4}\times\overline{r+8}}+\frac{3Bb^{6}}{\overline{r+4}\times\overline{r+8}}+\frac{3Bb^{8}}{\overline{r+4}\times\overline{r+8}} & +,\&c. = \frac{3B}{\overline{r+4}\times\overline{r+8}}\times\frac{b^{4}}{1-b^{2}} \\
+\frac{3Bb^{6}}{\overline{r+8}\times\overline{r+12}}+\frac{3Bb^{8}}{\overline{r+8}\times\overline{r+12}} & +,\&c. = \frac{3B}{\overline{r+8}\times\overline{r+12}}\times\frac{b^{6}}{1-b^{2}} \\
+\frac{3Bb^{8}}{\overline{r+12}\times\overline{r+16}} & +,\&c. = \frac{3B}{\overline{r+12}\times\overline{r+16}}\times\frac{b^{8}}{1-b^{2}} \\
\&c. & \qquad\qquad \&c.
\end{array}
$$

Ac proinde erit

$$S = \tfrac{3}{2}b + \tfrac{3}{2} \times \frac{5\times 7}{4\times 8}b^3 + \tfrac{3}{2} \times \frac{5\times 7}{4\times 8} \times \frac{9\times 11}{8\times 12}b^5 + \tfrac{3}{2} \times \frac{5\times 7}{4\times 8} \times \frac{9\times 11}{8\times 12} \times \frac{13\times 15}{12\times 16}b^7 +, \&c. + \frac{B}{1-b^2}$$

$$\times\overline{1 + \frac{3b^2}{r\times\overline{r+4}} + \frac{3b^4}{\overline{r+4}\times\overline{r+8}} + \frac{3b^6}{\overline{r+8}\times\overline{r+12}} + \frac{3b^8}{\overline{r+12}\times\overline{r+16}} +, \&c.}$$

Itaque ſi, computatis, exempli gratiâ, quindecim terminis, decimus ſextus deſignetur per B, erit $r = 15 \times 4 = 60$, et ſumma terminorum quindecim illorum addita ſummæ ſeriei $\frac{B}{1-b^2} \times \overline{1 + \frac{3b^2}{r\times\overline{r+4}} + \frac{3b^4}{\overline{r+4}\times\overline{r+8}} +, \&c.}$ dabit valorem coefficientis S.

Determinatis hoc pacto quantitatibus aſſumptis R, S, T, &c. jam ut ad expreſſiones virium revertamur, vis TR ad radium QS perpendicularis erat $\overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ ſin. QSP; ſed poſuimus angulum QSP $= \frac{1}{n}s$; eſtque $\frac{1}{z^3} = \frac{1}{t^3}$ in R + S coſ. $\frac{1}{n}s$ + T coſ. $\frac{2}{n}s$ + V coſ. $\frac{3}{n}s$ + W coſ. $\frac{4}{n}s$ +, &c.

Unde vis TR $= \frac{\varphi k}{t^3}$ in $\overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times$ ſin. $\frac{1}{n}s$ $+ \frac{S-V}{2}$ ſin. $\frac{2}{n}s + \frac{T-W}{2}$ ſin. $\frac{3}{n}s + \frac{V-X}{2}$ ſin. $\frac{4}{n}s$ +, &c.

Et vis quæ planetam Q diſtrahit à Sole in directione radii QS erat $\frac{\varphi}{z^3} \times \overline{k \text{ coſ. QSP} - 1} - \frac{\varphi}{t^3}$ coſ. QSP, hoc eſt, $\frac{\varphi}{t^3}$ in $\frac{kS}{2} - R + \overline{kR + \frac{kT}{2} - \frac{t^3}{k^2} - S}$

× coſ.

$$\times \text{coſ.}\ \frac{1}{n}s + \frac{kS+kV-2T}{2} \times \text{coſ.}\ \frac{2}{n}s + \frac{kT+kW-2V}{2}$$
$$\text{coſ.}\ \frac{3}{n}s + \frac{kV+kX-2W}{3}\ \text{coſ.}\ \frac{4}{n}s +, \text{ \&c. Q.E.I.}$$

PROPOSITIO II. PROBLEMA.

Inæqualitates motûs planetæ interioris ex viribus prædictis ortas inveſtigare.

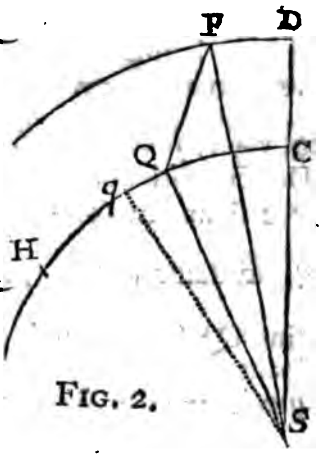


FIG. 2.

Exeant ſimul planetæ P, Q (Fig. 2.) de locis D, C, ubi jacebant in eâdem rectâ cum Sole poſito in S, et poſt aliquod temporis ſpatium reperiantur in P et Q, et jungantur SP, SQ, PQ. Eſto CS $= 1$, et arcus circularis CQ ſive angulus CSQ $= s$; denotent prætereà P et Q reſpectivè tempora periodica planetarum P et Q, eritque ang. QSC : ang. PSD :: P : Q, adeoque angulus QSP : ang. QSC :: P — Q : P, unde ang. QSP $= \frac{1}{n}s$, poſito $n = \frac{P}{P-Q}$.

Vis attractionis Solis ad diſtantiam QS, et tempus quo corpus, eâdem vi uniformiter agente, impulſum acquirere poſſet eam velocitatem, qua planeta Q in circulo CQ revolvitur, tùm illa ipſa velocitas, exponantur ſigillatim per unitatem; et ſi, ſumpto arcu CH $=$ CS $= 1$, CH exprimat tempus illud unitati æquale, arcus quilibet quàm minimus Qq exprimet tempus quo uniformi illâ velocitate deſcribitur.

Unde, cum velocitates viribus quibuſvis conſtantibus genitæ ſint ut ipſæ vires et tempora, quibus hæ velocitates generantur, conjunctim; erit velocitas 1 planetæ Q in circulo CQ revolventis ad incrementum vel decrementum velocitatis vi Z genitum (ſcripto nempe Z pro vi planetæ P normaliter ad radium QS agente, prout eſt in propoſitione præcedente definita) quo tempore planeta Q deſcribit arcum quàm minimum Qq, ut vis attractionis Solis 1 ducta in tempus CH ſive 1, ad vim Z ductam in tempus deſcriptionis arcûs Qq ſive in ipſum in arcum Qq: adeoque incrementum vel decrementum velocitatis vi Z genitum, quo tempore deſcribitur arcus Qq, exprimetur per Z × Qq ſive $Z \times \dot{s}$.

Eſt autem $Z = \frac{\varphi k}{t^3}$ in $\overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times$ ſin. $\frac{1}{n}s + \frac{S - V}{2}$ ſin. $\frac{2}{n}s + \frac{T - W}{2}$ ſin. $\frac{3}{n}s +$, &c. et hac quantitate ductâ in $\dot{s}$, tùm ſumptâ fluente, prodit velocitatis accceleratio ſive retardatio, quam voco U, genita quo tempore deſcribitur à planeta Q arcus CQ, æqualis $\frac{\varphi k n}{t^3}$ in $\overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times$ ſin. verſ. $\frac{1}{n}s + \frac{S - V}{4}$ ſin. verſ. $\frac{2}{n}s + \frac{T - W}{6}$ ſin. verſ. $\frac{3}{n}s + \frac{V - X}{8}$ ſin. verſ. $\frac{4}{n}s +$, &c. ſive poſito $b = R - \frac{t^3}{k^3} - \frac{T}{2} + \frac{S - V}{4} + \frac{T - W}{6} + \frac{V - X}{8} +$, &c. $U = \frac{\varphi k n}{t^3}$ in $b - \overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times$ coſ. $\frac{1}{n}s - \frac{S - V}{4}$ coſ. $\frac{2}{n}s - \frac{T - W}{6}$ coſ. $\frac{3}{n}s - \frac{V - X}{8}$ coſ. $\frac{4}{n}s -$, &c.

Hoc pacto obtinetur variatio velocitatis in hypotheſi quòd revolvatur planeta Q ſemper ad eamdem diſtantiam à Sole, quod in præcedenti calculo ſupponi poteſt, cùm tantillùm varietur diſtantia S Q actione planetæ P.

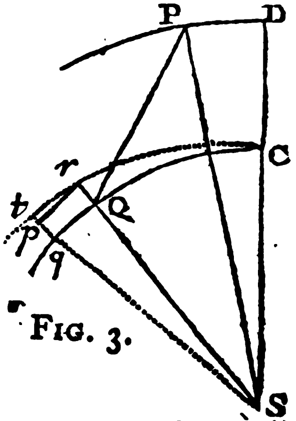


FIG. 3.

Hoc facto, ut inveſtigetur variatio diſtantiæ planetæ Q à Sole, fingamus planetam deſcripſiſſe, non arcum circularem C Q, ſed arcum curvæ C r (Fig. 3.) et reperiri in puncto r ubi radius S Q productus ſecat curvam.

Ducatur recta S t viciniſſima ipſi S Q occurrens circulo et curvæ q et t; tùm centro S et radio S r deſcribatur arcus rp, ſitque S r = x. Si planeta Q urgeretur ſolâ vi tendente ad centrum S, deſcriberet areas temporibus proportionales, atque adeò, cùm ipſius velocitas angularis in loco C ſupponatur eſſe 1, in loco r foret æqualis $\frac{1}{x}$; ſed in illo quem exhibet ſchema ſitu planetarum minuitur hæc velocitas quantitate U ſuprà definitâ, unde velocitas angularis in loco r erit $\frac{1}{x} - \mathrm{U}$; et tempus, quo deſcriberetur arcus Qq velocitate 1, eſt ad tempus quo deſcribitur arcus rp velocitate $\frac{1}{x} - \mathrm{U}$, ut Qq ad $\frac{rp}{\frac{1}{x} - \mathrm{U}}$, hoc eſt, ut $\grave{s}$ ad $\frac{x\grave{s}}{\frac{1}{x} - \mathrm{U}}$; unde, cùm $\grave{s}$ exprimat ex jam dictis tempus deſcriptionis arcûs Qq velocitate 1, exprimet quantitas $\frac{x\dot{s}}{\frac{1}{x}-U}$ tempus quo deſcribitur arcus rp velocitate $\frac{1}{x}-U$. His poſitis, quoniam planetæ Q receſſus à centro vel ad idem acceſſus pendet ex differentiâ virium, centrifugæ ſcilicet et centripetæ, quibus urgetur in Q; ſi hæc differentia virium dicatur P, et v denotet velocitatem aſcenſûs vel deſcenſûs planetæ Q ſecundum radium SQ, per idem planè ratiocinium, quod mox uſurpavimus in inveſtigatione velocitatis U, habebitur $\dot{v} = P \times \frac{x\dot{s}}{\frac{1}{x}-U}$.

Quoniam ex hypotheſi planeta Q, ſepoſitâ actione planetæ P, deſcriberet circulum, vires (centripeta et centrifuga) ſibi invicem et unitati forent æquales: exiſtente autem planetâ Q in r, ipſius attractio in Solem eſt $\frac{1}{x^2}$, ex qua auferenda eſt vis ea qua juxta propoſitionem præcedentem diſtrahitur à Sole, nimirum $\frac{a}{t^3}$ in $A + B$ coſ. $\frac{1}{n}s + C$ coſ. $\frac{2}{n}s + D$ coſ. $\frac{3}{n}s + E$ coſ. $\frac{4}{n}s +$, &c. poſitis $A = \frac{kS}{2} - R$, $B = kR + \frac{kT}{2} - \frac{t^2}{k^2} - S$, $C = \frac{kS+kV-2T}{2}$, $D = \frac{kT+kW-2V}{2}$, $E = \frac{kV+kX-2W}{2}$, &c. atque horum virium differentia componit vim centripetam.

Vis autem centrifuga eſt ſemper in ratione duplicatâ areæ temporis momento deſcriptæ directè et triplicatâ diſtantiæ inversè; unde ſi hæc vis fuerit æqualis

1, ubi incepit planeta movere in C, erit æqualis $x^2 \times \overline{\frac{1}{x} - U}^{\,2} \times \frac{1}{x^3} = \frac{1}{x} \times \overline{\frac{1}{x} - U}^{\,2}$ ubi movetur in r. Differentia igitur inter vim centrifugam et centripetam, qua urgetur planeta in r ſupra deſignata per P, eſt $\frac{1}{x} \times \overline{\frac{1}{x} - U}^{\,2} - \frac{1}{x^2} + \frac{\varphi}{t^3}$

$$\times \overline{A + B \operatorname{coſ.} \frac{1}{n} s + C \operatorname{coſ.} \frac{2}{n} s + D \operatorname{coſ.} \frac{3}{n} s +, \&c.}$$

hincque habetur $\dot{v} = \dot{s} \times \overline{\frac{1}{x} - U} - \frac{\dot{s}}{x \times \frac{1}{x} - U} + \frac{\varphi}{t^2}$

$$\times \frac{x\dot{s}}{\frac{1}{x} - U} \times \overline{A + B \operatorname{coſ.} \frac{1}{n} s + C \operatorname{coſ.} \frac{2}{n} s +, \&c.}$$

Vires, quibus perturbatur motus planetæ Q, cùm exprimantur ſeriebus quorum termini ducuntur in ſinum vel coſinum anguli $\frac{1}{n} s$, vel anguli hujus multiplicis, fingemus differentiam inter diſtantias SQ et Sr exprimi ſerie ſimili, ac propterea ponemus $x = 1 - Q + K \operatorname{coſ.} \frac{1}{n} s + L \operatorname{coſ.} \frac{2}{n} s + M \operatorname{coſ.} \frac{3}{n} s + N \operatorname{coſ.} \frac{4}{n} s$, &c. exiſtente $Q = K + L + M + N +$, &c. ut ſit Sr, ſive $x = 1$, ubi planetæ Q et P incipiunt movere à lineâ conjunctionis SCD. Quantitates autem aſſumptæ K, L, M, &c. ſunt exiguæ, ideoque erit $\frac{1}{x} = 1 + Q - K \operatorname{coſ.} \frac{1}{n} s - L \operatorname{coſ.} \frac{2}{n} s - M \operatorname{coſ.} \frac{3}{n} s - N \operatorname{coſ.} \frac{4}{n} s -$, &c. quamproximè.

proximè. Subſtituantur ergò in æquatione ſuprà traditâ valores quantitatum x, $\frac{1}{x}$, et U; et ſumptâ fluente, rejectis iis terminis qui ducuntur in altiorem quàm unam dimenſionem quantitatum φ, Q, K, L, &c. prodit $v = -\overline{\frac{2\varphi kbn}{t^3} - \frac{\varphi}{t^3}A - Q} \times s$

$$+\overline{\frac{2\varphi kn}{t^3} \times \overline{R - \frac{t^3}{k^3} - \frac{T}{2}} + \frac{\varphi}{t^3}B - K} \times n \times \text{ſin.}\, \frac{1}{n}s$$

$$+\overline{\frac{\varphi kn}{t^3} \times \frac{S - V}{4} + \frac{\varphi}{t^3} \times \frac{C}{2} - \frac{L}{2}} \times n \times \text{ſin.}\, \frac{2}{n}s$$

$$+\overline{\frac{\varphi kn}{t^3} \times \frac{T - W}{9} + \frac{\varphi}{t^3} \times \frac{D}{3} - \frac{M}{3}} \times n \times \text{ſin.}\, \frac{3}{n}s$$

$$+\overline{\frac{\varphi kn}{t^3} \times \frac{V - X}{16} + \frac{\varphi}{t^3} \times \frac{E}{4} - \frac{N}{4}} \times n \times \text{ſin.}\, \frac{4}{n}s +, \text{\&c.}$$

$+ Z$, deſignante Z quantitatem idoneam qua compleatur fluens. At, quoniam velocitas v ſupponitur nulla evadere, non ſolum ubi s, ſive arcus $CQ = 0$, id eſt, ubi planetæ verſantur in primâ illâ conjunctione, ſed etiam in omnibus aliis conjunctionibus ſubſequentibus, hoc eſt, ubi eſt angulus $\frac{1}{n}s$, ſeu $PSQ = 0$, vel $= r \times 180°$, ſcripto ſcilicet r pro quovis ex numeris naturalibus 1, 2, 3, 4, &c. fiet $Z = \overline{\frac{2\varphi kbn}{t^3} - \frac{\varphi}{t^3}A - Q} \times s$ adeoque

$$v = \overline{\frac{2\varphi kn}{t^3} \times \overline{R - \frac{t^3}{k^3} - \frac{T}{2}} + \frac{\varphi}{t^3}B - K} \times n \times \text{ſin.}\, \frac{1}{n}s$$

$$+\overline{\frac{\varphi kn}{t^3} \times \frac{S - V}{4} + \frac{\varphi}{t^3} \times \frac{C}{2} - \frac{L}{2}} \times n \times \text{ſin.}\, \frac{2}{n}s$$

$$+\overline{\frac{\varphi kn}{t^3} \times \frac{T - W}{9} + \frac{\varphi}{t^3} \times \frac{D}{3} - \frac{M}{3}} \times n \times \text{ſin.}\, \frac{3}{n}s$$

$+ \overline{\frac{\varphi k n}{t^3} \times \frac{V - X}{16} + \frac{\varphi}{t^3} \times \frac{E}{4} - \frac{N}{4}} \times n \times \text{ſin.}\, \frac{4}{n} s$ $+$, &c.

Deinde, cùm ſit tp, ſive $\dot{x}$ ad rp, ſive $x\dot{s}$, ut velocitas v qua deſcribitur tp ad velocitatem $\frac{1}{x} - U$ qua deſcribitur rp, erit $\dot{x} = v \times \frac{x\dot{s}}{\frac{1}{x} - U}$, ſive, quia valor velocitatis v componitur ex quantitatibus exiguis, $\dot{x} = v\dot{s}$ quamproximè, et $\frac{\dot{x}}{\dot{s}} = v$. Verùm etiam æquatio aſſumpta $x = 1 - Q + K \ \text{coſ.}\, \frac{1}{n} s + L \ \text{coſ.}\, \frac{2}{n} s + M \ \text{coſ.}\, \frac{3}{n} s +$, &c. dat $\frac{\dot{x}}{\dot{s}} = - K \times \frac{1}{n} \ \text{ſin.}\, \frac{1}{n} s - L \times \frac{2}{n} \ \text{ſin.}\, \frac{2}{n} s - M \times \frac{3}{n} \ \text{ſin.}\, \frac{3}{n} s - N \times \frac{4}{n} \ \text{ſin.}\, \frac{4}{n} s$, &c.

Habitis igitur duobus velocitatis v valoribus, eorum termini homologi ſtatuantur æquales, atque inde obtinebuntur quantitates aſſumptæ, nempe

$$K = \frac{\varphi}{t^3} \times \frac{n^2}{n^2 - 1} \times \overline{\overline{2kR - \frac{2t^3}{k^2}} \times \overline{n + \tfrac{1}{2}} - kT \times \overline{n - \tfrac{1}{2}} - S}$$

$$L = \frac{\varphi}{2t^3} \times \frac{n^2}{n^2 - 4} \times \overline{kS \times \overline{n + 1} - kV \times \overline{n - 1} - 2T}$$

$$M = \frac{\varphi}{3t^3} \times \frac{n^2}{n^2 - 9} \times \overline{kT \times \overline{n + \tfrac{3}{2}} - kW \times \overline{n - \tfrac{3}{2}} - 3V}$$

$$N = \frac{\varphi}{4t^3} \times \frac{n^2}{n^2 - 16} \times \overline{kV \times \overline{n + 2} - kX \times \overline{n - 2} - 4W}$$

&c.

indeque manifeſta fit harum quantitatum progreſſio:

atque hoc pacto habetur ſemper diſtantia x planetæ Q à Sole.

Jam ut definiatur planetæ Q motus verus qui deſignatur per s, dicatur w motus medius, ſive, quod perinde eſt, tempus quo planeta deſcripſerit arcum quemlibet Cr; atque ex demonſtratis eſt $\dot{w} = \frac{x\dot{s}}{\frac{1}{x} - \text{U}}$; unde, ſubſtitutis valoribus quantitatum, x, $\frac{1}{x}$, et U, et ſumptâ fluente, emergit

$$w = \overline{1 - 2\text{Q} + \frac{\varphi k b n}{3}} \times s + 2n\text{K} - \frac{\varphi k n^2}{t^3} \times \overline{\text{R} - \frac{t^3}{k^3} - \frac{\text{T}}{2}}$$
$$\times \text{ſin.}\, \frac{1}{n} s + \overline{n\text{L} - \frac{\varphi k n^2}{8 t^3} \times \overline{\text{S} - \text{V}}} \times \text{ſin.}\, \frac{2}{n} s$$
$$+ \overline{\frac{2 n \text{M}}{3} - \frac{\varphi k n^2}{18 t^3} \times \overline{\text{T} - \text{W}}} \times \text{ſin.}\, \frac{3}{n} s$$
$$+ \overline{\frac{n\text{N}}{2} - \frac{\varphi k n^2}{32 t^3} \times \overline{\text{V} - \text{X}}} \times \text{ſin.}\, \frac{4}{n} s +, \&c. + \text{Z}$$

denotante Z quantitatem idoneam ut compleatur fluens. Sed, quia motus verus medio æqualis evadere ſupponitur in qualibet planetarum P et Q conjunctione cum Sole, id eſt, ubi angulus PSQ ſive $\frac{1}{n} s$ æquatur, vel nihilo, vel angulo $r \times 180°$, exhibente r quemvis ex numeris naturalibus 1, 2, 3, 4, &c. erit $\text{Z} = \overline{2\text{Q} - \frac{\varphi k b n}{t^3}} \times s$. Ponantur igitur $\text{F} = -2n\text{K} + \frac{\varphi k n^2}{t^3} \times \overline{\text{R} - \frac{t^3}{k^3} - \frac{\text{T}}{2}}$, $\text{G} = -n\text{L} + \frac{\varphi k n^2}{8 t^3} \times \overline{\text{S} - \text{V}}$, $\text{H} = -\frac{2n\text{M}}{3} + \frac{\varphi k n^2}{18 t^3} \times \overline{\text{T} - \text{W}}$, $\text{I} = -\frac{n\text{N}}{2} + \frac{\varphi k n^2}{32 t^3}$

$\times$

$+ V - X$, &c. eritque motus verus, ſive $s = w + F \text{ ſin. } \frac{1}{n}s + G \times \text{ ſin. } \frac{2}{n}s + H \times \text{ ſin. } \frac{3}{n}s + I \times \text{ ſin. } \frac{1}{n}s +$, &c. vel, quia parum admodùm differt motus verus à motu medio $s = w + F \times \text{ ſin. } \frac{1}{n}w + G \times \text{ ſin. } \frac{2}{n}w + H \times \text{ ſin. } \frac{3}{n}w + I \times \text{ ſin. } \frac{4}{n}w +$, &c. Q. E. I.

COROLL. I.

His ita generatim definitis, ut ſpecialis eliciatur in motu cujuſpiam planetæ inæqualitatum menſura, determinandæ ſunt quantitates aſſumptæ.

Itaque planeta P deſignet Terram, planeta Q Venerem, et quoniam eſt diſtantia Terræ ad diſtantiam Veneris à Sole ut 100000 ad 72333, hæc erit ratio k ad 1, adeoque $k = \frac{100000}{72333}$, $kk + 1 = tt = 2.91129$, $b = \frac{2k}{t^3} = 0.94975$; atque inde per methodum in Prop. I^a^. expoſitam prodibunt

$R = 9.3925$ $V = 11.1964$ $Y = 5.3380$
$S = 16.6782$ $W = 8.8504$ $Z = 4.1029$
$T = 13.8877$ $X = 6.9045$ &c.

Tum, exiſtente periodo Terræ annuâ dierum 365.2565, et periodo Veneris dierum 224.701, eſt ex jam dictis $n = \frac{365.2565}{365.2565 - 224.701} = 2.59866$; et cùm gravitas in Solem ſit juxta Newtonum ad gravitatem in Terram, paribus diſtantiis, ut 1 ad $\frac{1}{169282}$ erit $\varphi = \frac{1}{169282}$.

Unde, redactis in numeros formulis in hac propoſitione datis, emergunt

$K = 0.0000103$
$L = 0.0000444$
$M = 0.0000377$
$N = -\ 0.0000065$
$O = -\ 0.0000024$
$O' = -\ 0.0000011$, &c.

Atque ex his tandem deducuntur

$F = -\ 0.0000473$
$G = -\ 0.0001078$
$H = -\ 0.0000684$
$I = 0.0000100$
$I' = 0.0000033$
&c.

Hinc ergo habentur valores coefficientium æquationis $s = w + F \times \text{ſin.}\ \frac{1}{n} w + G \times \text{ſin.}\ \frac{2}{n} w + H \times \text{ſin.}\ \frac{3}{n} w +$, &c. ubi s denotat motum Veneris verum, w motum medium, et $\frac{1}{n} w$ angulum PSQ ſive differentiam longitudinum heliocentricarum Terræ et Veneris; vel, reductis quantitatibus F, G, H, &c. ad exprimendas more aſtronomico circuli partes, fit

$$s = w - 9''.76 \times \text{ſin.}\ \frac{1}{n} w - 22''.24 \times \text{ſin.}\ \frac{2}{n} w + 14''.11 \times \text{ſin.}\ \frac{3}{n} w + 2''.06 \times \text{ſin.}\ \frac{4}{n} w + 0''.68 \times \text{ſin.}\ \frac{5}{n} w +, \text{ \&c.}$$

Ut exemplum apponam, eſto angulus PSQ ſive $\frac{1}{n} w = 40^\circ$, ac prodibit $s = w - 15''.5$; motus igitur medius ſuperat verum, eorumque differentia eſt $15''.5$.

Computatâ hoc pacto differentiâ inter motum Veneris verum et medium reſpectu Solis, ſequenti modo innoteſcet quanta evadat cùm e Terrâ ſpectatur. Eſto

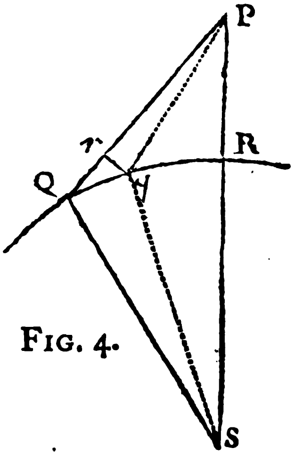


Fig. 4.

PSQ (Fig. 4.) angulus exhibens, ut priùs, differentiam longitudinum planetarum P et Q tempore quovis dato, et in circulo RQ exhibente portionem orbitæ planetæ Q, ſumatur arcus Qq æqualis differentiæ motuum prædictæ, et ductis Sq, Pq, centro P et radio Pq deſcribatur arcus qr ſecans PQ in r, atque, ob parvitatem arcuum Qq, qr, erit $Qq : qr$ :: rad. : ſin. PQq; deinde $\frac{Qq}{QS} : \frac{qr}{PQ}$ :: ang. QSq : ang. QPq; adeoque $\frac{\text{rad.}}{QS} : \frac{\text{ſin. } PQq}{PQ}$ :: ang. QSq : ang. QPq, unde ang. $QPq =$ ang. $QSq \times \frac{QS}{PQ} \times \frac{\text{ſin. } PQq}{\text{rad.}}$. Datis igitur angulo PSQ et diſtantiis PS, QS, dabitur diſtantia PQ, et angulus PQS, adeoque et angulus PQq: unde innoteſcet angulus quæſitus QPp, hoc eſt æquatio motûs, prout apparet ſpectatori in centro Terræ locato. Hincque, quamvis ſit modica motûs Veneris inæqualitas telluris actione genita, qualis tamen ſit ut pateat, libet eam in ſequenti tabulâ oculis ſubjicere.

Hujus tabulæ columna prima exhibet angulum QPS, ſive elongationem Veneris à Sole mediam; ſecunda indicat correctionem hujus elongationis, à conjunctione Veneris inferiore uſque ad maximam ejus elongationem quæ in orbe circulari eſt 46° 19′ 50″ circiter. Tertia et quarta columna eodem modo exhibent elongationem Veneris, ejuſque correctionem, à tempore elongationis maximæ uſque ad conjunctionem ſuperiorem.

Elongatio Ven. à Sole.	Correctio.	Elongatio Ven. à Sole.	Correctio.
° ′ ″	″	° ′ ″	″
0	0	46 19 50	0
5	0	46	+ 2.3
10	0	45	5.1
15	0	40	9.5
20	— 0.5	35	7.3
25	0.8	30	1.8
30	1.5	25	— 4.4
35	2.8	20	9.2
40	2.9	15	11.2
45	2.7	10	10.2
46	1.7	5	6.0
46 19 50	0	0	0

Exempli gratiâ, ſi Venus à conjunctione inferiore digreſſa motu ſuo medio diſceſſerit à Sole angulo elongationis 40°, erit vera Veneris elongatio $40^\circ - 2''.9 = 39^\circ\ 59'\ 57''.1$: pariter, ſi ulteriùs delata Venus pervenerit ad eamdem elongationem 40°, erit tunc vera Veneris elongatio $40^\circ\ 0'\ 9''.5$. Eædem omninò ſunt correctiones et cum iiſdem ſignis adhibendæ, ubi poſt conjunctionem ſuperiorem eædem eveniunt elongationes.

COROLL. II.

Ex præcedentibus etiàm deducitur diſtantia Veneris à Sole pro quolibet ejus cum Terrâ et Sole aſpectu,

in

in hypotheſi quod, ſecluſâ Terræ attractione, in orbitâ circulari revolveret. Sic, ſi angulus $\frac{1}{n}s$, ſeu PSQ ſit 90°, vel 270°, æquatio $x = 1 - Q + K \text{ coſ. } \frac{1}{n}s + L \text{ coſ. } \frac{2}{n}s + M \text{ coſ. } \frac{3}{n}s + N \text{ coſ. } \frac{4}{n}s +$, &c. fit $x = 0.9999437$ circiter; et ſi ſit PSQ $= 180°$, fit $x = 1.0000607$.

Unde, ſi diſtantia Veneris à Sole in conjunctione inferiore ponatur - - -	10000000
In quadraturis cum Terrâ erit ipſius diſtantia - - - - - - - - - - -	9999437
In conjunctione ſuperiore erit - - -	10000607

Item innoteſcit differentia inter tempus periodicum Veneris, quale nunc eſt, et tempus illud periodicum, quale foret, ſi unicâ Solis attractione in orbe circulari moveretur. Siquidem, cùm Venus poſt diſceſſum ſuum à conjunctione ad eamdem redierit, æquatio generalis in propoſitione tradita, quæ exprimit relationem inter motum Veneris verum et medium, evadit

$w = \overline{1 - 2Q + \frac{\phi khn}{t^3}} \times s$, ſive $w = 1.0000066 \times s$

circiter: unde tempus periodicum Veneris eſt ad tempus illud alterum periodicum, ut 1.0000066 ad 1; adeóque, ſi nulla foret gravitatio Veneris in Terram, revolutionem ſuam circa Solem minutis duobus horæ primis citiùs perageret.

PROPOSITIO III. PROBLEMA.

In ſyſtemate duorum planetarum in orbitis circularibus circa Solem revolventium, motum nodorum orbitæ planetæ interioris, quatenus ex vi planetæ exterioris oritur, inveſtigare.

Per motum nodorum hîc intelligendus eſt motus interſectionis plani orbis planetæ interioris cum plano orbis planetæ exterioris ſpectato ut immoto. Itaque eſto Sol in S (Fig. 5.) et centro S atque radio SQ deſcribantur in ſuperficie ſphæræ duo circuli QN, PN, ſeſe interſecantes in N, quorum prior QN deſignet ſitum plani orbis planetæ interioris Q, et poſterior PN ſitum plani orbis planetæ exterioris, cujus locus ſit in rectâ SP productâ. Eodem centro S et radio SP deſcribatur circulus PK, cujus planum ſit plano

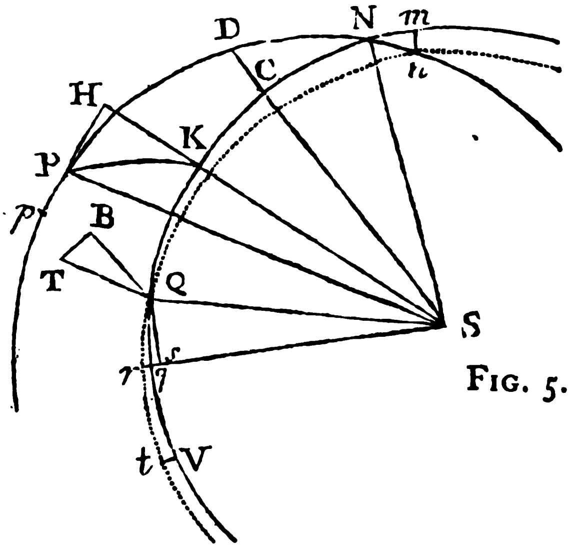


FIG. 5.

SQN perpendiculare, ſecetque circulum QN in K, et in SK demittatur perpendiculum PH: tum ductâ QT parallelâ rectæ SP et TB in planum SQN normali, ſi linea QT exhibeat vim qua trahitur planeta Q in directione QT, ſeu SP, TB exhibebit vim qua diſtrahitur perpendiculariter à plano ſuæ orbitæ; eritque triangulum QTB ſimile triangulo SPH, atque adeò, TB : QT :: PH : SP :: ſin. PK : 1; deinde in triangulo ſphærico rectangulo PKN habetur, 1 : ſin. PN :: ſin. PNK : ſin. PK; unde, conjunctis rationibus, et ſcripto c pro ſinu anguli PNK ad radium 1, hoc eſt, pro ſinu inclinationis orbis QN ad orbem PN, provenit $\mathrm{TB} = \mathrm{QT} \times c \times \text{ſin. PN}$. Sumatur jam arcus quàm minimus Qq, ad quem erigitur lineola perpendicularis qr, æqualis duplo ſpatio quod planeta Q percurrere poſſet impellente vi TB quo tempore in orbe ſuo deſcriberet arcum illum Qq; et centro S deſcriptus circulus rQn ſecans circulum PN in n exhibebit ſitum orbis planetæ Q poſt tempus illud, nodo N tranſlato in n; atque in QN demiſſo perpendiculo nm, et in Sq perpendiculo Qs, erit angulus qQr, ſive NQn ad duplum angulum qQs, id eſt, ad angulum QSq, ut vis TB ad gravitatem (nempe 1) planetæ Q in Solem; hoc eſt, $\frac{nm}{\text{ſin. QN}} : \mathrm{Q}q :: \mathrm{TB} : 1$; in triangulo autem rectangulo Nmn, eſt Nn : nm :: 1 : c; quarè conjunctis his rationibus, prodit $\mathrm{N}n = \frac{\mathrm{TB} \times \text{ſin. QN} \times \mathrm{Q}q}{c}$; ſed ſuprà invenimus $\mathrm{TB} = \mathrm{QT} \times c \times \text{ſin. PN}$, unde fit $\mathrm{N}n = \mathrm{QT} \times \text{ſin. PN} \times \text{ſin. QN} \times \mathrm{Q}q$.

Eſto SC linea conjunctionis planetarum, fiatque, ut in propoſitione præcedente, arcus $\mathrm{CQ} = s$, $\mathrm{Q}q = \dot{s}$, SQ $= 1$; et, quia inclinatio orbis QN ad orbem PN exigua ſupponitur, erit etiam hîc ang. PSQ $= \frac{1}{n}s$ quamproximè; proindeque, poſito arcu CN $= a$, erit QN $= s + a$ et PN $= s - \frac{1}{n}s + a$ quamproximè. Porrò, cùm lentiſſimè moveantur nodi, arcus CN ſpectari poteſt quaſi invariabilis per multarum planetæ Q revolutionum ſeriem, atque adeò fluxio arcus QN eadem erit cum fluxione arcûs QC. His poſitis, habebitur ſin. PN $\times$ ſin. QN $= \frac{1}{2}$ coſ. $\frac{1}{n}s - \frac{1}{2}$ coſ. $\overline{2s - \frac{1}{n}s + 2a}$, eſtque per propoſitionem primam QT $= \frac{\varphi k}{z^3} - \frac{\varphi}{k^2} = \frac{\varphi k}{t^3}$ in R $- \frac{t^3}{k^3} +$ S coſ. $\frac{1}{n}s +$ T coſ. $\frac{2}{n}s +$ V coſ. $\frac{3}{n}s +$ W coſ. $\frac{4}{n}s +$, &c. unde ſubſtitutis his valoribus in æquatione Nn $=$ QT $\times$ ſin. PN $\times$ ſin. QN $\times$ Qq, et ſumptâ fluente per methodum in Coroll. IV. lemmatis edoctam, prodibit ſumma omnium Nn, ſive motus nodi, quo tempore planeta Q à loco conjunctionis C procedens in orbe ſuo deſcripſerit arcum CQ, æqualis $\frac{\varphi k n}{2t^3}$ in $\frac{S}{2n}s + \overline{R - \frac{t^3}{k^3} + \frac{T}{2}} \times$ ſin. $\frac{1}{n}s + \frac{S+V}{4}$ ſin. $\frac{2}{n}s + \frac{T+W}{6}$ ſin. $\frac{3}{n}s +$, &c. $+ \frac{\varphi k n}{2t^3}$ in Z $\times$ ſin. $2a - \overline{R - \frac{t^3}{k^3}} \times \frac{1}{2n-1}$ ſin. $\overline{2s - \frac{1}{n}s + 2a} - \frac{S}{2} \times \frac{1}{2n}$ ſin. $\overline{2s + 2a} - \frac{S}{2} \times \frac{1}{2n-2}$ ſin. $\overline{2s - \frac{2}{n}s + 2a} - \frac{T}{2}$

$\times \frac{1}{2n-3}$ ſin. $\overline{2s - \frac{3}{n}s + 2a} - \frac{T}{2} \times \frac{1}{2n+1}$ ſin. $\overline{2s + \frac{1}{n}s + 2a} - \frac{V}{2} \times \frac{1}{2n-4}$ ſin. $\overline{2s - \frac{4}{n}s + 2a} - \frac{V}{2} \times \frac{1}{2n+2}$ ſin. $\overline{2s + \frac{2}{n}s + 2a}$, &c. exiſtente $Z = \overline{2n-1}$ in $R - \frac{t^3}{k^3} \times \frac{1}{\overline{2n-1}|^2} + \frac{S}{\overline{\overline{2n-2+2n}}} + \frac{T}{\overline{2n-3} \times \overline{2n+1}} + \frac{V}{\overline{2n-4} \times \overline{2n+2}} + \frac{W}{\overline{2n-5} \times \overline{2n+3}} +$, &c. atque in his ſeriebus patet terminorum progreſſio. Q. E. I.

Coroll. I.

Hic liquet multas oriri in motu nodorum æquationes; ſed quia minutæ ſunt, et locum planetæ Q ferè nihil mutant, ideò ſatis erit rationem habere motûs nodorum medii et æquationis ſolîus *periodicæ*, qui ſic ex præcedentibus deducuntur. Cùm in planis parùm ad ſe inclinatis moveri ſupponantur planetæ P et Q, quoties revertentur ad conjunctionem, angulus PSQ, ſive $\frac{1}{n}s$, qui metitur eorum diſtantiam à ſe invicem, evadet $= 360^\circ$ vel $= r \times 360^\circ$, exiſtente r numero integro: et quia, ſumpto arcu quolibet A, eſt ſemper ſin. $\overline{r \times 360^\circ + A} =$ ſin. A; hinc, ſi computatur motus nodi pro tempore conjunctionum, expreſſio illa generalis et prolixa in propoſitione tradita in hanc ſimplicem abit $\frac{\varphi k}{2t^3} \times \overline{\frac{S}{2}s - nZ \times \overline{\text{ſin.}\ \overline{2s + 2a} - \text{ſin.}\ 2a}}$, ſive per Coroll. I. lemmatis

$$\frac{\varphi k}{2t^3} \times \overline{\frac{S}{2}s - 2nZ \times \text{ſin.}\ s \times \text{coſ.}\ \overline{s + 2a}}.$$

Hic eſt igitur motus nodorum factus, quo tempore planetæ P et Q à conjunctione provecti poſt quotlibetcunque revolutiones ad conjunctionem quamvis aliam pervenerint, exhibente s arcum à planetâ Q in ſuâ orbitâ intereà deſcriptum. Terminus $\frac{\varphi k}{2t^3} \times 2nZ \times \text{ſin.}\, s \times \text{coſ.}\, \overline{s + 2a}$ indicat æquationem *periodicam*, et faciſlimè computatur: cùmque hæc æquatio modò ſit additiva, modò ſubtractiva, patet termino altero $\frac{\varphi k}{2t^3} \times \frac{S}{2} s$ exprimi generatìm motum nodi medium.

COROLL. II.

Eſto planeta P Terra, Q Venus, et revolutionem Veneris ab unâ conjunctione inferiore cùm Terrâ ad alteram vocemus, brevitatis gratiâ, revolutionem *ſynodicam*; eritque poſt unam revolutionem ſynodicam $\frac{1}{n} s = 360°$, proindeque $s = n \times 360° = 935°\ 31'$; hic igitur eſt arcus deſcriptus à Venere inter duas ejuſdem generis conjunctiones. Hinc motus nodi medius tempore revolutionis unius ſynodicæ, qui juxta corollarium præcedens eſt $\frac{\varphi k S}{4t^3} s$ fit $\frac{\varphi k n S}{4t^3} = 360° = 23''.087$; atque hic motus imminutus in ratione temporis periodici Terræ circa Solem ad revolutionem Veneris ſynodicam, id eſt, in ratione 1 ad $n - 1$, evadit $14''.44$, motus ſcilicet annuus nodorum Veneris regreſſivus, qui ſpatio centum annorum fit $24'\ 4''$.

Æquatio periodica $\frac{\varphi k n Z}{t^3} \times \text{ſin.}\, s \times \text{coſ.}\, \overline{s + 2a}$ ut adhuc ſimplicior evadat, ponamus arcum a ſive CN perexiguum eſſe vel nullum, id eſt, ſupponamus conjunctionem Terræ et Veneris fieri proximè in nodo, quemadmodum contingit hoc anno 1761, eritque æquatio periodica $\frac{\varphi k n Z}{t^3} \times \text{ſin.}\, s \times \text{coſ.}\, s = \frac{\varphi k n Z}{2 t^3} \times \text{ſin.}\, 2s$. Cùm igitur ſit $Z = 32.33$ circiter, formula $\frac{\varphi k S}{4 t^3} s - \frac{\varphi k n Z}{2 t^3}\, \text{ſin.}\, 2s$, quæ per corollarium præcedens exprimit generatìm motum nodi in qualibet ſerie revolutionum ſynodicarum confectum, fit $0.000006855 \times s - 14''.2 \times \text{ſin.}\, 2s$. Æquatio igitur periodica $14''.2 \times \text{ſin.}\, 2s$, quam *generalem* voco, eſt ut ſinus dupli arcûs à Venere deſcripti in datâ ſerie revolutionum ſynodicarum, nec ultra $14''.2$ aſcendit. Jam, ſi pro s ſubſtituatur $935^\circ\, 31'$, erit $\text{ſin.}\, 2s = \text{ſin.}\, 71^\circ\, 2'$, et regredientur nodi, in primâ revolutione ſynodicâ poſt conjunctionem factam in nodo, per arcum $23'' - 14''.2 \times \text{ſin.}\, 71^\circ\, 2' = 10''$: et, ſi r denotet numerum quemcumque revolutionum ſynodicarum, motus nodi, peractis illis revolutionibus, erit $r \times 23'' - 14''.2 \times \text{ſin.}\, r \times 71^\circ\, 2'$; pariterque, peractis revolutionibus quarum numerus eſt $r - 1$, idem motus erit $\overline{r - 1} \times 23'' - 14''.2 \times \text{ſin.}\, \overline{r - 1} \times 71^\circ\, 2'$; poſterior motus ex priore auferatur, et remanebit $23'' - 14''.2 \times \overline{\text{ſin.}\, r \times 71^\circ\, 2' - \text{ſin.}\, \overline{r - 1} \times 71^\circ\, 2'}$ $= 23'' - 14''.2 \times 2\, \text{ſin.}\, 35^\circ\, 31' \times \text{coſ.}\, \overline{r \times 71^\circ\, 2' - 35^\circ\, 31'}$ $= 23'' - 16''.5 \times \text{coſ.}\, 2r - 1 \times 35^\circ\, 31'$ pro motu nodi facto, tempore illius revolutionis ſynodicæ, cujus locum in ſerie revolutionum indicat numerus r. Exempli gratiâ, ſi deſideretur motus nodi tempore revolutionis quartæ ſynodicæ poſt conjunctionem factam in nodo, erit $r = 4$, et regreſſus nodi erit

$23'' - 16''.5 \times \text{coſ.}\ 7 \times 35^\circ\ 31 = 29''$. Sic ope hujus formulæ $23'' - 16''.5 \times \text{coſ.}\ \overline{2r - 1} \times 35^\circ\ 31'$ facilè computatur ſequens tabula, quæ exhibet regreſſum nodi Veneris in plano eclipticæ, pro duodecim ſigillatim revolutionibus ſynodicis quæ proximè ſequuntur conjunctionem Terræ et Veneris factam in nodo vel proximè ad nodum.

In revol. Ven. ſynod.	Regreſſus nodi Ven.	In revol. Ven. ſynod.	Regreſſus nodi Ven.
	″		″
1ª.	10	7ª.	26
2ª.	28	8ª.	39
3ª.	39	9ª.	30
4ª.	29	10ª.	11
5ª.	10	11ª.	8
6ª.	9	12ª.	25

Qui motus poteſt, cùm libuerit, ad annos communes reduci.

Denique patet æquationem periodicam, nempe $16''.5 \times \text{coſ.}\ \overline{2r - 1} \times 35^\circ\ 31'$, quam *ſpecialem* appello, ubi maxima eſt, evadere $16''\frac{1}{2}$; ac proinde regreſſum nodi in unâ revolutione ſynodicâ nuſquam ſuperare $39''\frac{1}{2}$, nec minorem eſſe $6''\frac{1}{2}$.

PROPOSITIO IV. PROBLEMA.

Iiſdem poſitis, variationem inclinationis orbis planetæ interioris ad planum orbis planetæ exterioris determinare.

Eſto NQV (Fig. 5.) quadrans circuli, cui erigatur perpendicularis Vt occurrens arcui nQr producto in t, eritque Vt menſura variationis inclinationis orbis NQV factæ quo tempore nodus N transfertur in n. Eſt autem $Vt : nm :: \text{ſin. } QV$ ſive $\text{coſ. } QN : \text{ſin. } QN$; atque $nm : Nn :: c : 1$, c denotante ſinum inclinationis orbis QN ad orbem PN, adeoque $Vt : Nn :: c \times \text{coſ. } QN : \text{ſin. } QN$; unde $Vt = Nn \times \frac{c \times \text{coſ. } QN}{\text{ſin. } QN}$, ſive, quia per propoſitionem ſuperiorem habetur $Nn = QT \times \text{ſin. } PN \times \text{ſin. } QN \times Qq$, $Vt = c \times QT \times \text{ſin. } PN \times \text{coſ. } QN \times Qq$. Hinc, cum ſit $\text{ſin. } PN \times \text{coſ. } QN = \frac{1}{2} \text{ ſin. } \overline{2s - \frac{1}{n}s + 2a} - \frac{1}{2} \text{ ſin. } \frac{1}{n}s$, ſumptâ fluente prodit variatio inclinationis, quo tempore planeta Q à loco conjunctionis C movetur per arcum CQ, æqualis $-\frac{\phi c k n}{2t^3} \text{ in } \overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times \text{ ſin. verſ. } \frac{1}{n}s + \frac{S - V}{4} \text{ ſin. verſ. } \frac{2}{n}s + \frac{T - W}{6} \text{ ſin. verſ. } \frac{3}{n}s + \frac{V - X}{8} \text{ ſin. verſ. } \frac{4}{n}s +$, &c. $+ \frac{\phi c k n}{2t^3} \text{ in } -Z \times \text{ ſin. verſ. } 2a + \overline{R - \frac{t^3}{k^3}} \times \frac{1}{2n-1} \text{ ſin. verſ. } \overline{2s - \frac{1}{n}s + 2a} + \frac{S}{2} \times \frac{1}{2n} \text{ ſin. verſ. } \overline{2s + 2a} + \frac{S}{2} \times \frac{1}{2n-2} \text{ ſin. verſ.}$

2 s

$$\overline{2s - \tfrac{2}{n}s + 2a} + \frac{T}{2} \times \frac{1}{2n+1} \text{ ſin. verſ. } \overline{2s + \tfrac{1}{n}s + 2a}$$

$$+ \frac{T}{2} \times \frac{1}{2n-3} \text{ ſin. verſ. } \overline{2s - \tfrac{3}{n}s + 2a} + \frac{V}{2} \times \frac{1}{2n+2}$$

$$\text{ſin. verſ. } \overline{2s + \tfrac{2}{n}s + 2a} + \frac{V}{2} \times \frac{1}{2n-4} \text{ ſin. verſ.}$$

$$\overline{2s - \tfrac{4}{n}s + 2a} + \frac{W}{2} \times \frac{1}{2n+3} \text{ ſin. verſ. } \overline{2s + \tfrac{3}{n}s + 2a}$$

$$+ \frac{W}{2} \times \frac{1}{2n-5} \text{ ſin. verſ. } \overline{2s - \tfrac{5}{n}s + 2a}, \text{ \&c.}$$

Exiſtente hîc eodem valore quantitatis Z ac in propoſitione præcedente. Q. E. I.

COROLL.

Si computetur variatio inclinationis pro tempore conjunctionum, facilè obtinebitur; hæc enim per formulam in propoſitione traditam evadit $\frac{\varphi c k n}{2t^3} \times Z \times \overline{\text{ſin. verſ. } \overline{2s + 2a} - \text{ſin. verſ. } 2a}$ quæ itèm, ſi prima conjunctionum, à qua ſumitur motûs exordium, ſtatuatur in nodo, fit $\frac{\varphi c k n}{2t^3} \times Z \times$ ſin. verſ. $2s$.

Hoc eſt igitur decrementum inclinationis orbis planetæ Q factum in qualibet ſerie revolutionum ad conjunctionem, deſignante s arcum intereà à planetâ circa Solem deſcriptum. Conferatur hæc inclinationis variatio cum æquatione nodi periodicâ eodem tempore genitâ, prout in propoſitione ſuperiore definitur, et patebit priorem eſſe ad poſteriorem ut $c \times$ ſin. verſ. $2s$ ad ſin. $2s$.

Ut ad orbem Veneris hæc transferantur, quem ſi inclinari ad orbem Terræ ſupponatur angulo 3° 23′ 20″;

erit $\frac{\phi c k n}{2t^3} \times Z \times$ ſin. verſ. $2s = 0''.84 \times$ ſin. verſ. $2s$. Unde palàm fit: 1°. in quacumque ſerie revolutionum ſynodicarum, poſt conjunctionem factam in nodo, decrementum inclinationis orbitæ Veneris ad eclipticam non ſuperare $2 \times 0''.84 = 1''.68$, quod è Terrâ ſpectatum evadit $4''.4$: 2°. cùm, peractâ unâ revolutione ſynodicâ, ſit ſin. verſ. $2s =$ ſin. verſ. $71^\circ\, 2'$, inclinationis decrementum pro qualibet ſerie revolutionum ſynodicarum quarum numerus eſt r, eſſe $0''.84 \times$ ſin. verſ. $r \times 71^\circ\, 2'$, et pro ſerie revolutionum quarum numerus eſt $r - 1$, eſſe $0''.84 \times$ ſin. verſ. $\overline{r - 1} \times 71^\circ\, 2'$; unde horum decrementorum differentia $0''.84 \times \overline{\text{ſin. verſ. } r \times 71^\circ\, 2' - \text{ſin. verſ. } \overline{r - 1} \times 71^\circ\, 2'} = 0''.84 \times 2\ \text{ſin. } 35^\circ\, 31' \times \text{ſin. } \overline{2r - 1} \times 35^\circ\, 31' = 0''.98 \times \text{ſin. } \overline{2r - 1} \times 35^\circ\, 31'$, exprimit variationem inclinationis genitam tempore revolutionis ſynodicæ illius, cujus locum in ſerie revolutionum denotat numerus r: atque hæc variatio, ut patet, nuſquam excedit $0''.98$ è Sole conſpecta, quæ ſpectatori in centro Terræ collocato ſub angulo $2''\frac{1}{4}$ apparebit. Cum igitur tantilla ſit orbitæ Veneris inclinationis variatio, non videtur operæ pretium de eâ ulteriùs exquirere.

Demonſtratis, quæ ad perturbationem motûs planetæ interioris ſpectant, ſupereſt, ut, quibus perturbationibus afficiatur motus planetæ exterioris, viciſſim expendamus.

PROPOSITIO V. PROBLEMA.

In ſyſtemate duorum planetarum circa Solem in orbibus penè circularibus revolventium, determinare vim planetæ interioris ad perturbandum motum exterioris.

Simili ratiocinio ei, quod in propoſitione primâ uſurpavimus, etiam hoc problema ſolvitur. Itaque poſitâ unitate pro diſtantiâ planetæ P à Sole, ubi ambo planetæ P et Q conjunguntur cum Sole, (Fig. 1.) fiat $SP = x$, $SQ = k$, $PQ = z$. Sit 1 ad φ ut gravitatio planetæ P in Solem in diſtantiâ 1 ad ejuſdem planetæ P gravitationem in planetam Q in eâdem diſtantiâ, eritque $\frac{\varphi}{z^2}$ gravitas planetæ P in planetam Q in diſtantiâ PQ. Productâ, ſi opus eſt, PQ ad O ut ſit $PO = \frac{\varphi}{z^2}$, et ductâ OI parallelâ rectæ QS occurrente PS productæ in I, reſolvatur vis PO in vires PI et OI, eritque propter ſimilia triangula PQS, POI, vis $OI = \frac{PO \times QS}{PQ} = \frac{\varphi k}{z^3}$, atque vis $PI = \frac{PO \times PS}{PQ} = \frac{\varphi x}{z^3}$ ſive vis $PI = \frac{\varphi}{z^3}$ quamproximè. Vis OI impellit planetam P in directione parallelâ rectæ SQ, et in eundem ſenſum urgetur Sol vi $\frac{\varphi}{k^2}$ qua gravitat in planetam Q: exceſſu igitur ſolo vis prioris ſupra poſteriorem, nempe $\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}$, cenſendus eſt urgeri planeta P in directione parallelâ rectæ SQ.

Porrò vis $\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}$ ea pars, quæ agit perpendiculariter ad radium PS, eſt $\overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ ſin. PSQ, atque altera pars, quæ amovet planetam P à Sole ſecundum PS, eſt $\overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ coſ. PSQ. Auferatur hæc poſterior vis ex vi PI, et manebit vis $\frac{\varphi}{z^3} + \overline{\frac{\varphi}{k^2} - \frac{\varphi k}{z^3}} \times$ coſ. PSQ, qua planeta P urgetur verſus Solem.

Eſto DCS (Fig. 2.) linea conjunctionis planetarum, et arcus DP, ſive angulus DSP vocetur s, denotentque P et Q reſpectivè tempora periodica planetarum P et Q, eritque, poſito $n = \frac{Q}{P - Q}$, ang. PSQ $= \frac{1}{n}s$. Tum, ſi fiat $t^2 = 1 + kk$, et $b = \frac{2k}{t^2}$, erit uti in Prop. I. expoſuimus, $z^2 = t^2 \times \overline{1 - b\,\text{coſ.}\,\frac{1}{n}s}$, atque $\frac{1}{z^3} = \frac{1}{t^3} \times \overline{R + S\,\text{coſ.}\,\frac{1}{n}s + T\,\text{coſ.}\,\frac{2}{n}s + V \times \text{coſ.}\,\frac{3}{n}s +, \text{\&c.}}$ et quemadmodùm ibi erat $b = \frac{2PS \times SQ}{\overline{PS}^2 + \overline{SQ}^2}$, hîc item eſt $b = \frac{2PS \times SQ}{\overline{PS}^2 + \overline{SQ}^2}$, adeoque valores quantitatum aſſumptarum R, S, T, &c. iidem hîc ſunt ac in propoſitione primâ.

Unde vis $\overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ ſin. PSQ, qua ſollicitatur planeta P in directione ad radium PS perpendiculari, ſic exprimetur $\frac{\varphi k}{t^3}$ in $\overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times$ ſin. $\frac{1}{n}s + \frac{S - V}{2}$

$\text{ſin. } \frac{2}{n}s + \frac{T - W}{2} \text{ ſin. } \frac{3}{n}s + \frac{V - X}{2} \text{ ſin. } \frac{4}{n}s +$, &c.

Et vis $\frac{\varphi}{z^3} + \overline{\frac{\varphi}{k^2} - \frac{\varphi k}{z^3}} \times \text{coſ. } PSQ$, qua urgetur planeta P in Solem ſecundum radium PS, fiet $\frac{\varphi}{t^3}$ in $R - \frac{kS}{2} - \overline{kR + \frac{kT}{2} - \frac{t^3}{k^2} - S} \times \text{coſ. } \frac{1}{n}s - \frac{kS + kV - 2T}{2} \text{ coſ. } \frac{2}{n}s - \frac{kT + kW - 2V}{2} \text{ coſ. } \frac{3}{n}s - \frac{kV + kX - 2W}{2} \text{ coſ. } \frac{4}{n}s +$, &c. Q. E. I.

PROPOSITIO VI. PROBLEMA.

Inæqualitates motûs planetæ exterioris ex viribus prædictis ortas inveſtigare.

Per analyſim in propoſitione ſecundâ inſtitutam vis ad radium PS perpendicularis generabit accelerationem, vel retardationem velocitatis, dum arcus quilibet DP deſcribitur à planeta P, æqualem $\frac{\varphi k n}{t^3}$ in $b - \overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times \text{coſ. } \frac{1}{n}s - \frac{S - V}{4} \text{ coſ. } \frac{2}{n}s - \frac{T - W}{6} \text{ coſ. } \frac{3}{n}s - \frac{V - X}{8} \text{ coſ. } \frac{4}{n}s -$, &c. $= U$ exiſtente $b = R - \frac{t^3}{k^3} - \frac{T}{2} + \frac{S - V}{4} + \frac{T - W}{6} + \frac{V - X}{8} +$, &c.

Deinde ſi ſcribatur p pro vi illâ planetæ Q qua urgetur planeta P in Solem, prout in propoſitione præcedente definita eſt, et v pro velocitate aſcenſûs vel deſcenſûs planetæ P ſecundum radium PS, et jam ſupponatur $SP = x = 1 - Q + K \text{ coſ. } \frac{1}{n}s + L \text{ coſ. } \frac{2}{n}s + M \text{ coſ. } \frac{3}{n}s + N \text{ coſ. } \frac{4}{n}s +$, &c. exiſtente $Q = K + L + M + N +$, &c. erit $\frac{1}{x^2} + p$ vis centripeta planetæ P, et $\frac{1}{x} \times \overline{\frac{1}{x} + U}|^2$ ejuſdem vis centrifuga, atque inde habebitur $\dot{v} = \overline{\frac{1}{x^2} + p - \frac{1}{x} \times \overline{\frac{1}{x} - U}|^2} \times \frac{x\dot{s}}{\frac{1}{x} + U}$.

Tum reſtitutis valoribus quantitatum U, p, x, et proſequendo calculum prout in Prop. II. poſitis

$$A = Kn + \frac{2\varphi k n^2}{t^3} \times \overline{R - \frac{t^3}{k^3} - \frac{T}{2}} - \frac{\varphi n}{t^3} \times \overline{kR - \frac{t^3}{k^2} - S + \frac{kT}{2}}$$

$$B = L \times \frac{n}{2} + \frac{\varphi k n^2}{4t^3} \times \overline{S - V} - \frac{\varphi n}{4t^3} \times \overline{kS + kV - 2T}$$

$$C = M \times \frac{n}{3} + \frac{\varphi k n^2}{9t^3} \times \overline{T - W} - \frac{\varphi n}{6t^3} \times \overline{kT + kW - 2V}$$

$$D = N \times \frac{n}{4} + \frac{\varphi k n^2}{16t^3} \times \overline{V - X} - \frac{\varphi n}{8t^3} \times \overline{kV + kX - 2W},$$

&c.

prodibit $v = \overline{\frac{\varphi}{t^3} \times \overline{R - \frac{kS}{2}} - \frac{2\varphi k b n}{t^3} - Q} \times s + A \times \text{ſin. } \frac{1}{n}s + B \times \text{ſin. } \frac{2}{n}s + C \times \text{ſin. } \frac{3}{n}s + D \times \text{ſin. } \frac{4}{n}s +$, &c. $+ Z$, et factâ hypotheſi quòd ſit $v = 0$ ubi angulus $PSQ = 0$, vel $r \times 180°$, exprimente r unum ex numeris naturalibus 1, 2, 3, 4, &c. erit $Z = -\overline{\frac{\varphi}{t^3} \times \overline{R - \frac{kS}{2}} - \frac{2\varphi k b n}{t^3} - Q} \times s$, ac proinde $v = \mathrm{A} \times \text{ſin.}\, \frac{1}{n} s + \mathrm{B} \times \text{ſin.}\, \frac{2}{n} s + \mathrm{C} \times \text{ſin.}\, \frac{3}{n} s + \mathrm{D} \times \frac{4}{n} s +$, &c.

Tùm, quia vis centripeta hîc excedere ſupponitur vim centrifugam, cùm contrarium ſuppoſitum fuerit in propoſitione ſecundâ, habetur $- \dot{x} = v \times \dfrac{x \dot{s}}{\frac{1}{x} + \mathrm{U}}$ ſive $- \dot{x} = v \dot{s}$ proximè, et $- \frac{\dot{x}}{\dot{s}} = v = \mathrm{K} \times \frac{1}{n}\, \text{ſin.}\, \frac{1}{n} s + \mathrm{L} \times \frac{2}{n}\, \text{ſin.}\, \frac{2}{n} s + \mathrm{M} \times \frac{3}{n}\, \text{ſin.}\, \frac{3}{n} s + \mathrm{N} \times \frac{4}{n}\, \text{ſin.}\, \frac{4}{n} s +$, &c.

Unde factâ collàtione terminorum hujus valoris velocitatis v cum terminis homologis valoris ſupra inventi, emergent

$$\mathrm{K} = -\frac{\varphi}{t^3} \times \frac{n^2}{n^2 - 1} \times \overline{\overline{2k\mathrm{R} - \frac{2t^3}{k^2}} \times \overline{n - \tfrac{1}{2}} - k\mathrm{T} \times \overline{n + \tfrac{1}{2}} + \mathrm{S}}$$

$$\mathrm{L} = -\frac{\varphi}{2t^3} \times \frac{n^2}{n^2 - 4} \times \overline{k\mathrm{S} \times \overline{n - 1} - k\mathrm{V} \times \overline{n + 1} + 2\mathrm{T}}$$

$$\mathrm{M} = -\frac{\varphi}{3t^3} \times \frac{n^2}{n^2 - 9} \times \overline{k\mathrm{T} \times \overline{n - \tfrac{3}{2}} - k\mathrm{W} \times \overline{n + \tfrac{3}{2}} + 3\mathrm{V}}$$

$$\mathrm{N} = -\frac{\varphi}{4t^3} \times \frac{n^2}{n^2 - 16} \times \overline{k\mathrm{V} \times \overline{n - 2} - k\mathrm{X} \times \overline{n + 2} + 4\mathrm{W}}$$

&c.

atque ità patet hujuſmodi quantitatum progreſſio. Innoteſcet igitur x, ſeu diſtàntia planetæ P à Sole in quovis ejus cum planetâ Q aſpectu.

Ut obtineatur planetæ P motus verus s, deſignet w motum medium, et cùm ſit $\dot{w} = \dfrac{x \dot{s}}{\frac{1}{x} + \mathrm{U}}$, ſubſtituantur valores quantitatum x, U, et ſumptâ fluente, poſitis

$$F = 2nK + \frac{\varphi k n^2}{t^3} \times \overline{R - \frac{t^3}{k^3} - \frac{T}{2}}$$

$$G = nL + \frac{\varphi k n^2}{8 t^3} \times \overline{S - V}$$

$$H = \frac{2nM}{3} + \frac{\varphi k n^2}{18 t^3} \times \overline{T - W}$$

$$I = \frac{nN}{2} + \frac{\varphi k n^2}{32 t^3} \times \overline{V - X}.$$

&c.

proveniet $w = \overline{1 - 2Q - \frac{\varphi k b n}{t^3}} \times s + F \times \text{ſin.}\ \frac{1}{n}s + G \times \text{ſin.}\ \frac{2}{n}s + H \times \text{ſin.}\ \frac{3}{n}s + I \times \text{ſin.}\ \frac{4}{n}s +$, &c. $+ Z$.

Et factâ hypotheſi quod motus verus coincidat cum medio ubi eſt $\frac{1}{n}s$, ſeu angulus PSQ $= 0$, vel $= r \times 180°$, exhibente r quemvis ex numeris 1, 2, 3, 4, &c. erit $Z = \overline{2Q + \frac{\varphi k b n}{t^3}} \times s$; ac proinde, ſcriptis $\frac{1}{n}w$, $\frac{2}{n}w$, &c. pro $\frac{1}{n}s$, $\frac{2}{n}s$, &c. quia parùm admodùm differt motus verus à medio, habetur motus verus, ſive $s = w - F \times \text{ſin.}\ \frac{1}{n}w - G \times \text{ſin.}\ \frac{2}{n}w - H \times \text{ſin.}\ \frac{3}{n}w - I \times \text{ſin.}\ \frac{4}{n}w -$, &c. Q. E. I.

COROLL. I.

Deſignet jam planeta P Terram, Q Venerem, et quia poſuimus eſſe diſtantiam mediocrem Terræ à

Sole ad diſtantiam mediocrem Veneris à Sole ut 1 ad k, erit hîc $k = 0.72333$, atque $t = \sqrt{1 + kk} = 1.234182$. Item eſt $n = \frac{Q}{P - Q} = \frac{224.701}{365.2565 - 224.701} = 1.59866$. Quantitates b, R, S, T, &c. eoſdem hîc retinent valores quos habebant in Coroll. I. Prop. II. Verùm, ut motuum Terreſtrium accurata inſtitueretur computatio, dignoſcere neceſſe eſſet effectus aliquos ab actione Veneris provenientes, ex quibus derivare liceret vim attractivam iſtius planetæ, ſed quia ſpeciales hujuſmodi effectus nulli, quantùm noverimus, obſervationibus aſtronomicis explorati habentur, propterea vim Veneris nunc conjecturâ definiemus, ut inde inæqualitates in motu Telluris computatæ, atque cum obſervationibus aſtronomicis collatæ inſervire poſthac poſſint ad eamdem vim certiùs determinandam. Itaque ſupponemus gravitatem in Solem eſſe ad gravitatem in Venerem, paribus diſtantiis, ut 400000 ad 1, hoc eſt, eſſe $\phi = \frac{1}{400000}$. Qui tamen valor vis ϕ ſi major vel minor poſteà deprehenſus fuerit, in eâdem ratione ſequentes omnes determinationes augendæ ſunt, vel minuendæ, adeoque ad juſtam menſuram facillimè reducentur. Erunt igitur

$K = -0.00000575$
$L = 0.00001643$
$M = 0.00000259$
$N = 0.00000090$
$O = 0.00000039$
$O' = 0.00000022$, &c.

Indeque colliguntur

$F = -0.00002459$
$G = 0.00002795$
$H = 0.00000345$
$I = 0.00000105$
$I' = 0.00000042$
&c.

atque reductis quantitatibus F, G, H, &c. in partes circuli, tandem habetur $s = w + 5''.07 \times \text{ſin.}\ \frac{1}{n} w - 5''.76 \times \text{ſin.}\ \frac{2}{n} w - 0''.71 \times \text{ſin.}\ \frac{3}{n} w - 0''.22 \times \text{ſin}\ \frac{4}{n} w -$, &c. ubi s denotat motum Terræ verum, w motum medium, et $\frac{1}{n} w$ angulum PSQ, ſive differentiam longitudinum heliocentricarum Terræ et Veneris.

Inde computatur ſequens tabula exhibens æquationem motûs Solis pro variâ diſtantiâ Veneris à Terrâ quam metitur angulus PSQ, ſive pro variâ differentiâ longitudinum heliocentricarum Terræ et Veneris quam metitur arcus circuli maximi inter Terram et Venerem interjectus et ſecundum ſeriem ſignorum à loco Terræ computatus.

Diſf.

Diff. long. hel. Terræ et Ven.	Æquatio motûs Solis.	Diff. long. hel. Terræ et Ven.	Æquatio motûs Solis.
°	″	°	″
Sig. 0. 0	— 0	Sig. VI. 0	— 0
10	1.6	10	2.6
20	2.8	20	5.0
30	3.4	30	7.0
Sig. I. 10	3.1	Sig. VII. 10	8.4
20	2.1	20	9.1
30	0.4	30	9.2
Sig. II. 10	+ 1.6	Sig. VIII. 10	8.6
20	3.8	20	7.5
30	5.8	30	5.8
Sig. III. 10	7.5	Sig. IX. 10	3.8
20	8.6	20	1.6
30	9.2	30	+ 0.4
Sig. IV. 10	9.1	Sig. X. 10	2.1
20	8.4	20	3.1
30	7.0	30	3.4
Sig. V. 10	5.0	Sig. XI. 10	2.8
20	2.6	20	0.6
30	0.	30	0

COROLL. II.

Si tellus gravitate ſuâ in Solem in circulo revolvi poſſe ſupponatur, adveniente Veneris actione variari debere diſtantiam ejus à Sole patet ex hac propoſitione. Eſto angulus $\frac{1}{n}s$, ſeu $PSQ = 90°$, vel $270°$, atque æquatio generalis $x = 1 - Q + K \text{ coſ. } \frac{1}{n}s + L \text{ coſ. } \frac{2}{n}s + M \text{ coſ. } \frac{3}{n}s +$, &c. in hanc abit $x = 0.9999693$; et ſi ſit $PSQ = 180°$, fit $x = 1.0000053$.

Unde ſi diſtantia Terræ à Sole, ubi verſatur in conjunctione cum Venere, ponatur - - - - - - - - - - -	10000000
In quadraturis cum Venere erit ipſius diſtantia - - - - - - - - - - -	9999693
Atque in oppoſitione - - - - - - -	10000053

PROPOSITIO VII. PROBLEMA.

In ſyſtemate duorum planetarum in circulis circa Solem revolventium, motum nodorum orbis planetæ exterioris in plano orbis planetæ interioris inveſtigare.

Eſto P locus planetæ exterioris (Fig. 5.) in orbe ſuo PN, SQ recta conjungens Solem et planetam interiorem, et dicatur c ſinus inclinationis duorum orbium ad ſe invicem ad radium 1, atque per propoſitionem quintam eſt $\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}$ vis qua planeta P amovetur ab orbe ſuo ſecundum directionem parallelam rectæ SQ, hujuſque vis ea pars quæ perpendiculariter agit in planum orbis PN, per ſimile ratiocinium quo uſi ſumus in Prop. III. prodit æqualis $c \times$ ſin. QN $\times \overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}}$, et motus interſectionis plani orbis PN cum plano orbis QN fit $\overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times$ ſin. PN $\times$ ſin. QN $\times$ Pp quo tempore planeta P deſcribit in orbe ſuo arcum quàm minimum Pp.

Deinde ſi deſignaverit D locum planetæ P ubi verſatur in conjunctione cum planetâ interiore, et ponantur DP $= s$, P$p = \dot{s}$, DN $= a$, erit PN $= s + a$, QN $= s + \frac{1}{n}s + a$ quamproximè, atque ſin. PN $\times$ ſin. QN $= \frac{1}{2}$ coſ. $\frac{1}{n}s - \frac{1}{2}$ coſ. $\overline{2s + \frac{1}{n}s + 2a}$.

Unde, calculum proſequendo uti in propoſitione tertiâ, motus nodorum factus, quo tempore planeta P à loco conjunctionis D diſcedens deſcripſerit in orbe ſuo arcum quemlibet DP, exprimetur per

$$\frac{\varphi k n}{2t^3} \text{ in } \frac{S}{2n}s + \overline{R - \frac{t^3}{k^3} + \frac{T}{2}} \times \text{ſin. } \frac{1}{n}s + \frac{S+V}{4} \text{ ſin. } \frac{2}{n}s + \frac{T+W}{6} \text{ ſin. } \frac{3}{n}s + \frac{V+X}{8} \text{ ſin. } \frac{4}{n}s +, \text{ \&c.}$$

$$+ \frac{\varphi k n}{2t^3} \text{ in } Z \times \text{ſin. } 2a - \overline{R - \frac{t^3}{k^3}} \times \frac{1}{2n+1} \text{ ſin. } \overline{2s + \frac{1}{n}s + 2a}$$

$$- \frac{S}{2} \times \frac{n}{2n} \text{ ſin. } \overline{2s + 2a} - \frac{S}{2} \times \frac{1}{2n+2} \text{ ſin. } \overline{2s + \frac{2}{n}s + 2a}$$

$$- \frac{T}{2} \times \frac{1}{2n-1} \text{ ſin. } \overline{2s - \frac{1}{n}s + 2a} - \frac{T}{2} \times \frac{1}{2n+3} \text{ ſin. } \overline{2s + \frac{3}{n}s + 2a} - \frac{V}{2} \times \frac{1}{2n-2} \text{ ſin. } \overline{2s - \frac{2}{n}s + 2a}$$

—

$- \frac{V}{2} \times \frac{1}{2n+4} \sin. \overline{2s + \frac{4}{n}s + 2a} - \frac{W}{2} \times \frac{1}{2n-3} \sin. \overline{2s - \frac{3}{n}s + 2a} - \frac{W}{2} \times \frac{1}{2n+5} \sin. \overline{2s + \frac{5}{n}s + 2a}$, &c. exiſtente $Z = 2n + 1$ in $R - \frac{t^3}{k} \times \frac{1}{\overline{2n+1}^{\,2}} + \frac{S}{2n \times \overline{2n+2}} + \frac{T}{\overline{2n-1} \times \overline{2n+3}} + \frac{V}{\overline{2n-2} \times \overline{2n+4}} + \frac{W}{\overline{2n-3} \times \overline{2n+5}} +$, &c. In quibus ſeriebus manifeſta eſt terminorum progreſſio. Q. E. I.

COROLL.

Hinc in conjunctionibus expreſſio motûs nodi evadit $\frac{\varphi k}{2t^3} \times \overline{\frac{S}{2}s - nZ \times \overline{\sin. \overline{2s + 2a} - \sin. 2a}}$. Hicque eſt motus nodi factus quo tempore planetæ P et Q à conjunctione procedentes ad conjunctionem quamvis aliam pervenerint, exhibente s arcum à planetâ P in ſuâ orbitâ intereà deſcriptum. Terminus $\frac{\varphi k}{2t^3} \times \frac{S}{2}s$ exprimit motum nodi medium, et terminus alter $\frac{\varphi kn}{2t^3} Z \times \overline{\sin. \overline{2s + 2a} - \sin. 2a}$ indicat æquationem *periodicam generalem*; vel etiam, ſi conjunctio illa à qua deſumitur computationis initium, fieri ſupponatur in nodo, vel propè ad nodum, æquatio periodica generalis fit $\frac{\varphi kn}{2t^3} Z \times \sin 2s$.

Deſignet jam planeta P Terram, Q Venerem, eritque poſt unam revolutionem ſynodicam, id eſt, poſt revolutionem Veneris ad Terram, $\frac{1}{n}s = 360°$, proindeque $s = \bar{n} \times 360° = 575° \, 31'$. Quare motus nodi medius huic temporis ſpatio congruens fit $\frac{\phi k n}{4t^3} S \times 360°$, qui imminutus in ratione revolutionis Terræ circa Solem ad ejuſdem revolutionem ad Venerem, hoc eſt, in ratione 1 ad n, evadit $\frac{\phi k}{4t^3} S \times 360°$ $= 5''.20$, motus ſcilicet nodi medius annuus quo regreditur interſectio planorum orbium Terræ ac Veneris; atque hic motus ſpatio centum annorum fit $8' \, 40''$.

In computo æquationis *periodicæ generalis* $\frac{\phi k n}{2t^3} Z \times$ ſin. $2s$, advertendum eſt omnes terminos, ex quibus componitur valor quantitatis Z, eoſdem hîc eſſe ac in Prop. III. præter terminum primum $\overline{R - \frac{t^3}{k^3}} \times \frac{1}{2n+1}$ qui ob diverſum valorem quantitatum t et k diverſus eſt. Hîc igitur provenit $Z = .3159$, adeoque $\frac{\phi k n}{2t^3} Z \times$ ſin. $2s = 5'' \times$ ſin. $2s$; unde patet æquationem hanc nunquam ſuperare $5''$. Motus igitur nodi verus, nimirùm $\frac{\phi k}{2t^3} \times \overline{\frac{S}{2} s - nZ \times \text{ſin. } 2s}$, peractâ unâ revolutione ſynodicâ poſt conjunctionem factam in nodo, evadit $8'.3 - 5'' \times$ ſin. $71°. \, 2'$, quia tunc eſt ſin. $2s =$ ſin. $2 \times 575°. \, 31' =$ ſin. $71°. \, 2'$; et per ratiocinium ſimile ei, quod in Coroll. II. Prop. III. uſurpatum eſt, conſtabit $8''.3 - 5''.8 \times$ coſ. $\overline{2r - 1} \times 35°. \, 31'$ exprimere regreſſum nodi factum tempore illius revolutionis ſynodicæ, cujus loeum in ſerie revolutionum indicat numerus r. Hinc computatur tabula ſequens quæ exhibet regreſſum nodi orbitæ Terreſtris in plano orbis Veneris pro duodecim ſigillatìm revolutionibus ſynodicis quæ proximè ſequuntur conjunctionem Terræ et Veneris factam in nodo, vel proximè ad nodum.

In revol. ſynod.	Regreſſus nodi Ter.	In revol. ſynod.	Regreſſus nodi Ter.
	″		″
1	4	7	9
2	10	8	14
3	14	9	11
4	10	10	4
5	4	11	3
6	3	12	9

Patet autem æquationem *periodicam ſpecialem*, nempe $5''.8 \times \text{coſ.}\ \overline{2r - 1} \times 35^\circ.\ 31'$, ubi maxima eſt, evadere $5''.8$, et regreſſum nodi in quavis revolutione Terræ ad Venerem non aſſurgere ultra $14''$, nec minui citra $2''\frac{1}{2}$.

Propositio VIII. Problema.

Iiſdem poſitis, variationem inclinationis orbis planetæ exterioris ad planum orbis planetæ interioris determinare.

Deſignet I variationem inclinationis factam quo tempore planeta P deſcribit arcum quàm minimum

Pp, et N motum nodi eodem tempore confectum, ac per ratiocinium omnino ſimile ei quod adhibitum eſt in propoſitione quartâ habetur $I = N \times \frac{c \times \text{coſ. PN}}{\text{ſin. PN}}$: ſed per propoſitionem præcedentem eſt $N = \overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times \text{ſin. PN} \times \text{ſin. QN} \times Pp$, adeoque fit $I = \overline{\frac{\varphi k}{z^3} - \frac{\varphi}{k^2}} \times c \times \text{coſ. PN} \times \text{ſin. QN} \times Pp$.

Unde, cùm hîc ſit $PN = s + a$, $QN = s + \frac{1}{n}s + a$, proindeque coſ. PN × ſin. QN $= \frac{1}{2}\,\text{ſin.}\,\frac{1}{n}s + \frac{1}{2}\,\text{ſin.}\,\overline{2s + \frac{1}{n}s + 2a}$, ſumptâ fluente prodit variatio inclinationis genita, quo tempore planeta deſcripſerit in orbe ſuo arcum quemlibet DP à loco conjunctionis D, æqualis $\frac{\varphi ckn}{2t^3}$ in $\overline{R - \frac{t^3}{k^3} - \frac{T}{2}} \times \text{ſin. verſ.}\,\frac{1}{n}s + \frac{S - V}{4}\,\text{ſin. verſ.}\,\frac{2}{n}s + \frac{T - W}{6}\,\text{ſin. verſ.}\,\frac{3}{n}s + \frac{V - X}{8}\,\text{ſin. verſ.}\,\frac{4}{n}s +$, &c. $+ \frac{\varphi ckn}{2t^3}$ in $- Z \times \text{ſin. verſ.}\,2a + \overline{R - \frac{t^3}{k^3}} \times \frac{1}{2n + 1}\,\text{ſin. verſ}$ $\overline{2s + \frac{1}{n}s + 2a} + \frac{S}{2} \times \frac{1}{2n}\,\text{ſin. verſ.}\,\overline{2s + 2a} +$ $\times \frac{1}{2n + 2}\,\text{ſin. verſ.}\,\overline{2s + \frac{2}{n}s + 2a} + \frac{T}{2} \times \frac{1}{2n -}$ ſin. verſ. $\overline{2s - \frac{1}{n}s + 2a} + \frac{T}{2} \times \frac{1}{2n + 3}$ ſin. v $\overline{2s + \frac{3}{n}s + 2a} + \frac{V}{2} \times \frac{1}{2n - 2}\,\text{ſin. verſ.}\,\overline{2s - \frac{2}{n}s +}$

$+ \frac{V}{2} \times \frac{1}{2n+4}$ ſin. verſ. $\overline{2s + \frac{4}{n}s + 2a}$, &c. Eumdem hîc habet valorem quantitas Z ac in propoſitione præcedente. Q. E. I.

COROLL.

Ubi angulus PSQ eſt nullus, vel multiplex anguli 360°, id eſt, ubi planetæ verſantur in conjunctione, variatio inclinationis genita generatim eſt $\frac{\varphi ckn}{2t^3} Z \times$ ſin. verſ. $\overline{2s + 2a}$ — ſin. verſ. $2a$ quæ, ſi ponatur arcus DN $= a = 0$, fit $\frac{\varphi ckn}{2t^3} Z \times$ ſin. verſ. $2s$.

Atque hoc eſt decrementum inclinationis orbis planetæ P ad orbem planetæ Q factum in qualibet ſerie revolutionum ad conjunctionem, initio ſumpto à conjunctione factâ in nodo, vel prope ad nodum, et deſignante s arcum intereà à planetâ P in orbe ſuo deſcriptum.

Si inde computetur decrementum inclinationis orbis Terreſtris ſupra planum orbitæ Veneris factum poſt quotcumque revolutiones Veneris ad Terram, fiet $\frac{\varphi ckn}{2t^3} Z \times$ ſin. verſ. $2s = 0''.3 \times$ ſin. verſ. $2s$, adeoque hoc decrementum, ubi maximum evadit, non ſuperat $0''.6$, ac proinde in omni caſu negligi poteſt.

XLIII. *An Account of a Treatiſe in* French, *preſented to the Royal Society, intituled,* " Lettres ſur l'Electricité, *by the Abbé* " Nollet, *Member of the Royal Academy* " *of Sciences*, &c. &c." *By* William Watſon, *M. D. R. S. S.*

Gentlemen,

Read Dec. 17, 1761.

ABOUT eight years ſince, the learned and ingenious author of the work before us publiſhed a treatiſe, of which the preſent work may be conſidered as a continuation. That conſiſted of nine letters upon the ſubject of electricity, which were addreſſed to perſons, who had diſtinguiſhed themſelves by their endeavours to illuſtrate this part of natural philoſophy. In like manner, the preſent performance conſiſts of eight letters, and is addreſſed, as the former, to his friends and correſpondents.

As an account of the former treatiſe was communicated by myſelf to the Royal Society, and printed, by direction of the council, in the Philoſophical Tranſactions *, the author requeſts, at the end of the ſixteenth letter, which is addreſſed to me, that I would give myſelf the additional trouble to lay before you an account of the preſent work. This requeſt I moſt readily comply with, not only in obedience to the order of the Society, but likewiſe as a teſtimony of the eſteem and regard, which I have long entertained, and ſhall continue to do, for the excellent author of it.

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* Vide Vol. XLVIII. p. 201.

The principal deſign of the work before us, is to ſupport, and further confirm, the hypotheſis of the author, and of ſeveral other perſons, who have conſidered theſe matters, *that the effects of electricity depend upon the ſimultaneous affluence and effluence of the electric matter.* This treatiſe, like the former, is printed in 12mo. and contains 284 pages, excluſive of the preface, and four tables, exhibiting fourteen figures.

In defending his opinions, in relation to the effects of electricity, the Abbé Nollet has given a variety of new experiments, which cannot but be agreeable to thoſe, who are converſant in theſe matters. He has alſo occaſionally mentioned thoſe of other perſons, which are come to his knowledge, and which he apprehends not to be ſufficiently known. He has traced the origin of ſeveral happy inventions, and has exhibited to us the real authors of them. He has given, as he imagines, additional value to ſeveral experiments, which appear to him to have been too much neglected; and brought others, which have been over-rated, to their proper ſtandard.

As this work is of a controverſial kind, the author has had particular attention to ſuch points, as have been the occaſion of conteſt; to weigh the reaſons of his opponents, and to add new explanations to ſuch of his opinions, as ſeemed to want them; more particularly, to ſuch as have appeared to him to have been miſunderſtood.

The firſt of theſe letters is addreſſed to M. Necker, profeſſor of experimental philoſophy at Geneva. In this letter, our author endeavours to eſtabliſh his opinion, publiſhed long ſince, in regard to the exiſtence of the ſimultaneous affluence and effluence, and conſequently the double current, of the electric matter, in oppoſite directions. And herein our author, by a ſeries of experiments, obviates ſome doubts, which had occurred to Mr. Necker, in relation to the validity of this hypotheſis.

The ſecond letter is addreſſed, as the former was, to M. Necker of Geneva. In this letter, the hypotheſis of M. Jallabert of Geneva, a very worthy member of this Society, in relation to the electrical phænomena, is examined; and ſuch part of it, as does not coincide with the ideas of our author, he endeavours to confute by an ingenious ſeries of deductions.

The third, fourth, and fifth Letters are addreſſed to M. Du Tour, of Riom in Auvergne, who has been a diligent enquirer into the nature and properties of electricity. In the firſt of theſe, is a careful examination of the validity of the doctrine of *plus* and *minus* in bodies electrified. So early as in February 1745, I communicated to the Royal Society an experiment, and ſome deductions therefrom, which laid the foundation of this doctrine. This experiment, and the deductions in conſequence of it, were afterwards printed in the Philoſophical Tranſactions *. Theſe I explained more at large, both by experiments and obſervations, in another paper, read to the Society in February 1745-6 †, and were the experiments, which ſo early cauſed me to conceive, that there was ſomething in the phænomena of electricity, not to be reſolved, but upon ſtatical principles; and enabled me firſt to aſſert, that the phænomena in bodies electriſed, however ſimilar they might appear, did really ariſe *from their electricity being either greater or leſs than their natural quantity*. This doctrine has, ſince that time, been the cauſe of a vaſt variety of experiments, both here and abroad, by which great light has been thrown upon this part of natural philoſophy. How far our author has been able to overturn this doctrine, muſt be left to other judges to determine.

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* Vide Vol. XLIV. p. 739.

† See Phil. Tranſ. Vol. XLV. p. 93—101.

In the fourth letter, the doctrine of reſinous and vitreous electricity is examined. In this letter, as well as in the fifth, a great number both of experiments and deductions are produced, not only to weaken the doctrine of *plus* and *minus*, but to eſtabliſh the principle of *ſimultaneous affluence* and *effluence* of electric matter; as, if this principle is allowed, the doctrine of reſinous and vitreous electricity may be reduced to it: as our author is of opinion, that there is only one and the ſame kind of electricity, whether it is natural or artificial; and that, however appearances may make it ſeem to vary, the electricity is one and the ſame.

The ſixth letter is an anſwer to one of Father Beccaria, profeſſor of experimental philoſophy in the univerſity of Turin, publiſhed in Italian, in the year 1753, and addreſſed to the Abbé Nollet. This letter of Pere Beccaria was tranſlated into French, and publiſhed at Paris in 1754, by M. Delor, with many additions and annotations. It contains a very great number of curious experiments and obſervations,

both

both upon artificial and natural electricity; many of which are brought to prove the validity of the doctrine of our worthy member Dr. Franklin, in oppoſition to that of the Abbé Nollet. More particularly, he endeavours to confute the abbé's opinion, in relation to the affluence of the electric matter, which the abbé has, by experiments and obſervations, ingeniouſly endeavoured to confirm. Pere Beccaria's obſervations upon natural electricity, and upon meteors, on which he has made a prodigious number of experiments, many of them of a delicate nature, do him a great deal of honour.

The ſeventh letter, the ingenious author does me the honour to addreſs to me. In this letter, he, with juſtice, laments the calamities of war; more particularly, as it, in a great degree, prevents that correſpondence between men of letters, which contributes ſo much to their mutual ſatisfaction, and upon which the improvement of ſcience ſo much depends. The more particular purport of this letter, is to anſwer ſome objections, which Mr. David Colden, of North America, publiſhed againſt the former letters of our author. Theſe relate more particularly to the impermeability of glaſs to the electric fluid, and to the explanation of the phænomena of the experiment of Leyden. Beſides theſe, he gives us his idea of non-electriſed bodies electriſed *plus*, as he does not approve of the idea generally received of the *accumulation* of electricity. He mentions, that he has read Mr. Canton's memoir *relating to electricity, with his obſervations upon ſtormy clouds*. He finds many curious facts in that work; but thinks them not ſufficient to make the deductions Mr. Canton has done, in favour of the doctrine of *plus* and *minus*. M. Du Tour of Riom, has ſent the Abbé Nollet a memoir, which he has likewiſe been ſo kind as to ſend me, containing a review of theſe experiments, from which he thinks it very eaſy to reſolve all theſe phænomena, upon the doctrine of ſimultaneous affluence and effluence of the electric matter.

The eighth letter is addreſſed to M. De Romas, aſſeſſor to the preſidial of Nerac, and contains remarks upon electrical kites; upon Father Ammerſin's manner of preparing and uſing wood to *inſulate* bodies, in making electrical experiments; and likewiſe ſome obſervations concerning the doctrine of ſimultaneous affluence and effluence of the electric matter. M. De Romas, in flying his electrical kite, was the firſt who uſed a cord compoſed of hemp and wire. This compounded cord conducted the electricity of the clouds far more perfectly than a hempen cord would do, even though it was wetted; and this cord being terminated by one of dry ſilk, enabled the obſerver, by a proper management of the apparatus, to make what experiments he thought proper, without danger to himſelf. The Abbé Nollet, however, deſires M. De Romas to be very cautious in making theſe experiments, and not too much to confide in his ſilk lines; as the vaſtneſs of the electrical matter in thunder-ſtorms may overcome the property of the ſilk, and even make it a conductor of electricity, and hazard the life of the obſerver. The quantity of electricity brought by M. De Romas's kite from the clouds has been ſo great, that, on the 26th of Auguſt 1756, " the ſtreams of " fire were an inch thick, and ten feet long, which " were conducted by the cord of the kite to the

" non-electric bodies near it, and the report of which " was equal to that of a piſtol." If a ſtroke of this kind had gone through the body of M. De Romas, probably the late unfortunate Profeſſor Richmann had no longer been the only martyr to electricity.

Father Ammerſin's method of preparing wood, ſo as to make it ſerve the purpoſe of glaſs, wax, &c. in electrical experiments, was publiſhed at Lucerne in the year 1754, and our author has given us an extract of it at the end of his work. This father found, that the frying of wood, after its being well dried in an oven, or otherwiſe, in either the oil of walnuts or that of linſeed, made it fit to *inſulate* thoſe bodies, which you choſe to electriſe, by preventing the diſſipation of the electricity: not only ſo, but what makes it ſtill more valuable to thoſe, who are engaged in theſe purſuits, you may excite electricity with it, as the Abbé Nollet ſays he has done, to his great convenience. He ſays further, that the end of a board mounted upon four pegs, a pair of wooden ſhoes, ſome truncheons of beech, walnut, or lime, &c. fried in oil, coſt him but little, and anſwered his purpoſe better than cakes of wax, pitch, roſin, and all the ſupports of glaſs or ſilk, which he had employed before: and, in caſe of neceſſity, a cylinder of this prepared wood, or a globe turned out of it, will excite an electricity ſo ſtrong, that you need not be at the trouble of exciting it with other bodies. Father Ammerſin himſelf employs common wooden meaſures, ſuch as are uſually found in granaries, firſt boiled in oil, and afterwards mounted ſo as to be turned by his wheel.

The Abbé Nollet, being deſirous of ſupporting the validity of ſome opinions of his, in relation to the nature and properties of electricity, deſired of the Royal Academy, that a committee ſhould be appointed, to examine the truth of ſome experiments, which the abbé conſidered as proofs of what he had eſtabliſhed. A committee was accordingly appointed, which conſiſted of Meſſieurs Deparcieux, Fougeroux, Bezout, Tillet, and Briſſon, who all atteſted to the academy, that the reſults of theſe experiments, at the making of which they were preſent, were ſuch as the abbé had foretold, in a memoir, which had been read to the academy; an atteſtation of which is given in this work, ſigned by M. De Fouchy, ſecretary to the academy, and is dated 10th April 1760.

Theſe experiments are ſixty in number, ſome of which are ſubdivided to more ſubordinate ones, and are moſt of them exceedingly well choſen. They tend to prove the ſimultaneous affluence and effluence of the electric matter, a doctrine long ſince eſpouſed, and very well ſupported by our author; but vehemently, and with much aſperity, controverted by ſome gentlemen at Paris. For a detail of theſe experiments, I muſt refer you to the work itſelf; and as they without doubt are very fairly ſtated, every perſon converſant in theſe enquiries will carefully conſider them, and, at the ſame time, reflect how far the hypotheſis is deducible from the phænomena.

I am, with the moſt profound reſpect,
Gentlemen,
Your moſt obedient
humble ſervant,

May 24, 1761. W. Watſon.

LIV. *The Caſe of a Man, whoſe Heart was found enlarged to a very uncommon Size, by Mr.* Richard Pulteney: *Communicated by* W. Watſon, *M. D. R. S. S.*

Read Dec. 17, 1761.

THO. C. aged about thirty-two or thirty-three years, had the rickets in his infancy, and continued very weakly for ſeveral years after. In the winter of the year 1759, upon taking cold, he was afflicted with peripneumonic and pleuritic ſymptoms; which had ſcarcely left him, when he was ſeized in the ſummer of the year 1760, after great exerciſe in walking, with a fever, and very violent rheumatiſm: this, after affecting moſt of his joints, remained the longeſt and moſt troubleſome in his knees. When he was ſomewhat better of his rheumatiſm, but before the pain and ſtiffneſs of his joints had left him, he was adviſed to go into the cold bath: he did ſo; but, upon coming out again, inſtantly felt ſuch an increaſed load, fainting, and anxiety about the præcordia, that he thought he ſhould ſcarcely have recovered the ſhock it gave him: nevertheleſs, he ventured in again a day or two after; but experienced the former ſymptoms, in an aggravated degree; and from this time dated the diſorder which terminated his life. A palpitation of the heart, to which he had been ſubject for ſome years before, became now much ſtronger, and gradually increaſed with his other complaints, to a very great degree. His rheumatiſm continued to affect his breaſt, and all his joints, particularly his knees; eſpecially, upon taking cold, or any irregularity in the non-naturals, he became weaker, breathed ſhorter, eſpecially upon walking a little, or talking rather more or higher than uſual, any of which exertions put him out of breath preſently.

When he firſt applied to me, in the beginning of March 1761, I found him labouring under the abovementioned complaints; and upon examining his pulſe, found it ſoft, and extremely quick: it commonly went at the rate of 110 in the morning, and in the evening 120, pulſations in a minute, as I repeatedly obſerved. The palpitation of the heart ſtruck me inſtantly, as it ſhook his whole body at every ſtroke. I could never obſerve any inequality of the intermittent kind in the pulſe, under any the moſt accelerated motion thereof, or in whatſoever ſituation the body was placed.

At this time the chylopoietic organs were all tolerably good. Stimulating food, or fermented liquors, had, for ſome time, always increaſed his anxiety and load upon his breaſt, and this experience had induced him to refrain from them.

He had ſlept very ill for ſeveral months, ſometimes not more than an hour or two during the whole courſe of the night. He could not ſleep on the left ſide at all, and was always eaſieſt in an erect poſture. He was commonly awaked with a ſenſe of ſuffocation, from the vaſt load and oppreſſion upon his breaſt, and from the ſtrength of the palpitation.

From his firſt application to me, I had no hopes of doing him any real ſervice, as I thought it evident, from his complaints, and particularly from the great and uninterrupted palpitation, and the feel of the pulſe, that there was ſomething very extraordinarily diſordered in the heart itſelf, or in ſome of the large veſſels near it. The regularity of the pulſe inclined me to ſuppoſe an aneuriſm, rather than polypoſe affections. All this time, however, no outward appearance ſtrengthened this ſuppoſition.

No remedies alleviated his complaints in any degree, except bleeding, which afforded a relief; but very temporary, and weakened him too much to be repeated more than once. All that it ſeemed to do for him was the procuring him rather more ſleep the night after, than he uſually had, and eaſing a little tickling cough which had remained with him, ever ſince the year 1759, at times; and particularly ſince his rheumatiſm, but which was never very troubleſome.

Soon after I firſt ſaw him his legs became œdematous, and by the beginning of April his thighs were much enlarged, and at length his belly in ſome degree. At this time he began to cough more from having taken cold, inadvertently as he thought, but he ſoon expectorated freely. By the middle of April he was too weak to ſit up, nor could he ſpeak or ſtir without being ready to expire for want of breath. On the night of the 20th of April, as he was coughing an hæmoptöe ſuffocated him inſtantly.

About two quarts of a thin coffee-coloured liquor were found in the cavity of the abdomen. The omentum was very ſmall, perhaps it would not weigh more than two ounces. The ſtomach and inteſtines were greatly inflated. In all other reſpects, the viſcera of this cavity, as far as an haſty examination would permit us to obſerve, were in a ſound ſtate.

In the thorax we found the lungs very ſound, but extremely turgid with blood: they adhered very firmly to the pleura on both ſides, and particularly on the left, where the adheſion was almoſt total. The heart, as might be expected, appeared to be the organ principally affected. The pericardium adhered almoſt every-where ſo cloſe, as to form, as it were, the external coat thereof. The heart itſelf was of an enormous ſize, and of a very pale colour, and looſe and flaccid in its texture, to a very remarkable degree. As far as I could judge, from the moſt careful examination and compariſon, I could not find that either of the auricles or ventricles bore an extraordinary proportion to the other. The whole heart might be ſaid to be entirely aneuriſmatical. The parietes were every-where thin, in proportion to the ſize of the whole. There was no particular enlargement of the aorta, as far as I traced it, which I did to ſome diſtance; but its texture, as that of the heart, was very lax and flabby. I could not find the leaſt polypoſe concretions in any part whatſoever. When the heart was cut ſhort from the great veſſels, emptied of the coagula, and waſhed as clean as poſſible, it weighed upwards of twenty-eight ounces avoirdupoiſe weight.

OBSERVATIONS.

The ſize of the human heart, in a natural ſtate, is known to differ greatly in different ſubjects. Diſſections prove this beyond all controverſy, and it is uſually ſuppoſed, that the capacity of the blood-veſſels bears a general proportion to the ſize and capacity of the heart itſelf (1). Very few anatomiſts, in deſcribing this organ, have eſtimated its ſize by its weight. Dr. Haller (2), where he treats ſo amply and profeſſedly upon the heart, does not, from his own knowledge, mention its weight. From Tabor, he ſays, it is eſtimated at ten ounces; but this is ſuppoſed to be when freed from the auricles, as well as the extremities of the larger veſſels. Its mean weight by ſome other anatomiſts is reckoned at thirteen ounces.

Aneuriſms of the heart, both with and without polypoſe concretions, are not unfrequent; many inſtances occur in the writers of obſervations. Dr. Douglas (3) ſaw a young man, who died of a palpitation of the heart, the left ventricle of which was found three times larger than the right. This caſe bears a conſiderable analogy to the inſtance before us; and is quoted, among ſeveral others, by the Baron Van Swieten, in treating upon aneuriſms of the heart (4). The baron alſo relates a caſe from Lanciſi, in which the left ventricle was twice as large as the right; and the whole heart weighed two pounds and an half. Hoffman, in his ſyſtema, when treating upon the palpitation of the heart, gives us a caſe, where the heart was greatly diſtended; but he does not aſcertain to what degree, by any method whatever: he only ſays, *cor miræ fuit magnitudinis* (5).

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(1) Hoffman. Opera omnia, Tom. I. lib. i. cap. vi. De Sanguinis Circuitu. Suppl. II. Part. iii. p. 65. Hiſt. Corp. Human. Anatom. § 641.

(2) Element. Phyſiolog. Vol. I. p. 326.

(3) Phil. Tranſ. abridged by Jones, Vol. V. p. 229.

(4) Comment. in Aphor. Vol. I. ad ſect. 176.

(5) Opera omnia, Tom. III. p. 92.

De Haen, in his Ratio Medendi (6), tells us, he was preſent at the opening of a man, whoſe heart was three times bigger, at leaſt, than in its natural ſtate. The dilatation was in its left ventricle, which was ſo thin as to reſemble a whitiſh membrane only; and the heart was broader at its apex than at its baſe.

De Haen likewiſe, in his ratio Medendi (7), informs us, that the heart of a woman, who died of a fever, with extreme debility, weighed twenty-four ounces, even after it was waſhed, and wiped very dry. This increaſed weight and magnitude aroſe more particularly from the left ventricle. The extenſion of ventricles was ſo great, that they both together contained more than a quart. Though this woman was no more than thirty-ſeven years of age, the aorta at its baſe was degenerated into bone, and was four inches in circumference. Beſides the whole portion of the aorta at its baſe being oſſified, there were interſperſed in ſeveral parts of its length, what our author calls *inſulæ oſſeæ*. In one, who lived ſo long as the excellent Wepfer, ſuch appearances are not extraordinary; but in one ſo little advanced as the woman in queſtion, theſe oſſifications are very unuſual.

It would be endleſs to quote inſtances of the preternatural dilatation of this organ: to name no more, we have a very recent and ſtriking one of this kind, in the body of our late Moſt Gracious Sovereign, whoſe ſudden death was owing to the rupture of the right ventricle of the heart: a circumſtance, which cannot be conceived to have taken place, without a previous gradual dilatation of the ſame, and that, probably, to a very conſiderable degree.

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(6) Cap. xxx. De Aneuryſmate.

(7) Pars ſexta, p. 143.

In caſes of this kind, commonly one of the ventricles is found diſtended to a monſtrous ſize, while the reſt of the heart remains nearly in its natural ſtate. It is but rare, perhaps, that the heart is ſeen ſo equally and univerſally enlarged, as in the caſe under conſideration.

This man, I have obſerved, had the rickets, when a child: in this diſorder, the whole ſyſtem is found to be in a very lax debilitated ſtate; and the heart is ſaid to be ſo in particular. The conſtitutions of rickety children frequently amend as they grow up, and particularly about the age of puberty. But, in this caſe, I think we may ſafely conclude, that this man's heart never recovered its due tone, after he grew up. It is ſcarcely to be ſuppoſed, that the heart could ſuffer ſo great an enlargement during the laſt year or two of his life only: the more ſo, as I remember to have heard him ſay, that, for many years before his death, a very little exerciſe put him out of breath. Doubtleſs it was increaſed greatly during the latter years of his life, by his buſineſs, which obliged him to exerciſe much, particularly in walking; ſo that before he got his rheumatiſm, he came home ſo weak, and ſo much fatigued with his uſual day's exerciſe, that he has been almoſt unable to ſtir for a day or two. We may add to this, the increaſed force that the heart ſuſtained during the time he laboured under his inflammatory diſorders, both before and after his rheumatiſm ſeized him.

The great increaſe of his diſorder, upon going into the cold bath, is not ſurprizing. The ſhock of the cold water, and the reſiſtance neceſſarily given, by that means, to the circulation, muſt occaſion a vaſt ſurcharge of blood in the auricles and ventricles of the heart, already too weak to perform its office with ſufficient power. Beſides the impropriety of ſuch a ſtep, while there was reaſon to think, that the inflammatory ſpiſſitude of the blood was by no means overcome, the preternatural diſtention was doubtleſs increaſed by this means.

From hence, however, may be deduced an uſeful hint in practice; namely, where, from the ſtate of the pulſe, from a palpitation of the heart, a faint weak voice, an aptitude to fall into lipothymies from ſlight cauſes, or from the concurrence of any other ſymptoms, we have reaſon to ſuſpect, that the heart is too weak; in ſuch caſes, not to direct cold bathing, until the patient has been prepared for it, by going into water between the degrees of tepid and quite cold water; nay, probably, it might be better to wait, before cold bathing be preſcribed at all, till the effect of medicines ſeems previouſly to have invigorated, in ſome degree, the cardiac ſyſtem.

The conſidering the heart as a muſcle capable, like all others, of great alteration reſpecting its tone; and, at the ſame time, that ſuch alteration muſt eſſentially affect the whole animal œconomy, from the very great importance of the organ itſelf, is evidently of great uſe in medicine. It muſt aſſiſt us in accounting for ſeveral phænomena that occur in various diſorders, which are utterly inexplicable by other means; and of conſequence, muſt lead to a more ſucceſsful practice. In nervous diſorders, and in Fevers of the putrid malignant kind for inſtance, we find the heart ſo extraordinarily weakened, that it is in many inſtances dangerous to ſubject the patient to an erect poſture, even though it be but for a very little time (8). Syncopes and even fatal deliquia and comatoſe affections have been the conſequence. In ſcurvies too where the whole ſyſtem is become very lax and tender, and has loſt much of its tonic and vital elaſticity, the ſame phænomena have occurred (9). In theſe caſes the neceſſity of the horizontal, or at leaſt the recumbent poſture, is manifeſt; as it is obvious how much more force is requiſite to throw the blood up into the head in an erect than in an horizontal poſition.

It is probable that the extreme weakneſs and ſlow recovery of ſome women, particularly ſuch as are of a delicate conſtitution, after a hard labour depends often upon the weakneſs of the heart, occaſioned by the force it ſuſtained during the throws of labour. In theſe caſes, though reſt is among the firſt methods of recovery, yet I think I have obſerved the uſe of the quinquina to be attended with good ſucceſs.

To conclude, it is probable that caſes of this kind occur much oftener than we are aware of; as, doubtleſs, the diſſection of morbid bodies, were that but more frequently allowed of, would teach us. There is room to think, that this is the caſe, though not in the degree of the inſtance before us, in almoſt all diſeaſes ariſing from a weak and lax fibre. Cheſelden tells us, in his Anatomy, that in perſons " that died " of a dropſy, he always obſerved the heart large, its

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(8) Vide Hoffman. Opera, Tom. II. p. 72. Tom. VI. p. 169. De Situ erecto in Morbis periculoſis valde noxio.

(9) Engalen. De Scorbuto, p. 226. et paſſim.

" fibres lax; and the veſſels about it immoderately " diſtended."

Ariſtotle (10) expreſly ſays, that timid people, and thoſe of cold conſtitutions, have large hearts; on the contrary, that the bold, and thoſe of a warm temperament, have ſmall ones. Nor does this opinion of that excellent philoſopher ſeem ill founded; as women, children, and weakly men, from whom much courage is not looked for, are lax-fibred, and, conſequently, more liable to an enlargement of this organ, than thoſe of the human ſpecies, who are robuſt and tenſe fibred, from whom a manly exertion of courage is more to be expected.

LV. *An Account of ſeveral Experiments in Electricity: In a Letter to Mr.* Benjamin Wilſon, *F. R. S.* *By* Edward Delaval, *Eſq*; *F. R. S.*

S I R, Old Palace-yard, June 8, 1761.

Read Dec. 17, 1761.

IT appears by the experiments mentioned in my letter to you, publiſhed in the fifty-firſt volume of the Philoſophical Tranſactions, that ſtones, and other earthy ſubſtances, are convertible by ſeveral methods, and particularly by different degrees of heat, from non-electrics into electrics.

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(10) Lib. iii. De Partib. Animal. cap. iv.

Since that time, I find it has been the opinion of ſome perſons, that this change does not *immediately* depend on the heat, but only *conſequentially*, by evaporating the moiſture, which, they ſuppoſe, returns again on the bodies cooling.

This ſuppoſition will naturally, at firſt view, preſent itſelf to every one, who conſiders the *beginning only* of thoſe experiments; but I did not think any careful obſerver, who had repeated them, or conſidered all the circumſtances of them, would have been miſled by it.

That you may judge the better of this, I ſhall mention the circumſtances of one of thoſe experiments particularly. When a common tobacco-pipe, or any other ſlender body of the like kind, is heated red-hot, it conducts the electric fluid as perfectly as when cold: on cooling, it gradually arrives at its moſt perfect electric ſtate in two minutes; and, in leſs than two minutes more, it entirely loſes its electric property again, though at that time it is not cold: it cannot, therefore, in that interval, have imbibed a moiſture ſufficient to have deſtroyed its electricity. Nor are any of the ſubſtances, employed in the experiment, of that kind of bodies, which are apt ſuddenly to draw moiſture from the air.

In confirmation of particular bodies requiring particular degrees of heat, to render them electric or non-electric, independent of moiſture, I ſhall acquaint you with a ſubſtance, which is affected by heat in an oppoſite manner to the former inſtances; for the degree of heat neceſſary to render the *other* ſubſtances electric, makes *this* non-electric.

The ſubſtance I am ſpeaking of is *iſland cryſtal*, (which is well known for its ſingular property of a double refraction) on a piece of which, I have made the following obſervations. 1ſt, After this piece of cryſtal has been rubbed, when the heat of the air is moderate, it ſhews ſigns of electricity, though not very ſtrong ones: 2d obſ. If the heat is increaſed, ſo as to be a little greater than that of the hand, it deſtroys its electric power entirely: 3d obſ. By cooling the ſtone again, the electric power is reſtored.

I immerſed *this* piece of cryſtal into a veſſel filled with quickſilver, and ſurrounded by ice, where it remained near two hours, when the weather was very cold: upon taking it out with a pair of tongs, (that it might not be altered by the heat of my hands) and rubbing it again, it was more ſtrongly electric than I had at any other time experienced; but, on placing it for a few minutes on the hearth, at ſome diſtance from the fire, its electric property was again deſtroyed, for rubbing would not occaſion any ſigns thereof.

Thus we ſee two different kinds of *fixed* bodies, the one of which acquires an electric property, with the ſame heat, with which another loſes it; while a third ſet of ſubſtances, as glaſs, &c. retain their electricity, through both the degrees of heat, neceſſary to the other two.

Some pieces of iſland cryſtal, which I have procured from different places, *have not the property of loſing their electricity by a moderate heat.* I have, in particular, a piece of that cryſtal, one part whereof, when gently heated, becomes non-electric, while the other part with the ſame heat (or even with a much greater one) remains perfectly electric.

There are ſeveral other earthy ſubſtances, I find, whoſe electricity is deſtroyed by very different degrees of heat.

From conſidering, that the degree of heat, at which the iſland cryſtal, firſt mentioned in this letter, is in its moſt perfect electric ſtate, is leſs than the uſual heat of the air; and that a ſmall increaſe of that heat renders it non-electric; I do not think it improbable, that many ſubſtances, which are not known to be electric, may prove ſo, if expoſed to a greater degree of cold than they have hitherto been examined in.

I am,

S I R,

Your moſt humble ſervant,

E. Delaval.

LVI. *An Account of an Encrinus, or Star-fiſh, with a jointed Stem, taken on the Coaſt of* Barbadoes, *which explains to what kind of Animal thoſe Foſſils belong, called Starſtones, Aſteriæ, and Aſtropodia, which have been found in many Parts of this Kingdom: In a Letter to Mr.* Emanuel Mendes da Coſta, *F. R. S. By* John Ellis, *Eſq; F. R. S.*

SIR,

Read Dec. 17, 1761.

I Need not inform you, that the writers on natural hiſtory have been much at a loſs to diſcover to what kind of animals thoſe petrified bodies have properly belonged, which are known to us by the name of trochites, entrochi, carpophylloides, encrini, aſteriæ, &c. and therefore, it is with the greater pleaſure I lay before the Royal Society a recent animal of the rareſt of this claſs.

Mr. Maſon of Barbadoes, remarkable for his curious experiments in magnetiſm, by deſire of my friend Dr. Alexander Bruce, of that iſland, in the month of May 1760, brought me this rare lithophyton, as the doctor called it; but I being in the country, it fell into the hands of my worthy friend Dr. John Fothergil, who was ſo kind to ſend it me, to deſcribe, and to oblige the Royal Society with a ſight of it.

Dr. Bruce informs me, that they are the inhabitants of thoſe ſeas, and that he is in hopes of ſending me over a more perfect ſpecimen.

Mr. Guettard, that able and curious naturaliſt, has given, in the Memoirs of the Academy of Sciences at Paris, publiſhed in 1761, for the year 1755, a moſt minute deſcription and diſſection of an animal of this kind, from the curious cabinet of Madam Bois Jourdain of Paris; it was ſent from Martinico by the name of palma marina; the head of it, being more perfect than ours, has ſome reſemblance to the branches of a palm tree.

However, as there is ſome little difference in the figure of both theſe animals, and as I, about a year ago, had the honour of exhibiting to the Royal Society a curious drawing of it, which Dr. Gartner, of Stutgart in Wurtenburg, F. R. S. drew for me, I ſhall give the deſcription that occurred to me, upon the beſt examination I could take of it, without diſſecting, or breaking the ſpecimen.

As it comes neareſt to the foſſils called encrini, or lilii lapidei, I ſhall ſtill keep that name, and call it

Encrinus, Capite ſtellato ramoſo-dichotomo,
Stipite pentagono equiſetiformi.

The ſtem and head of this animal, in its preſent ſtate, meaſures about fourteen inches. The ſtem is about thirteen inches in height, and about the third of an inch in diameter, leſſening a little towards the top: it is formed of pentagonous joints, or vertebræ, placed regularly over one another, which are of a teſtaceous ſubſtance, and united by very thin cartilages; as appears, by examining minutely the baſe of the loweſt vertebra, where it is faſtened to the ſtarry indentures of the joint: this makes the vertebræ capable of bending at the will of the animal, in any direction.

If we examine the five furrows or channels along the ſtem, we ſhall diſcover a ſmall hole between every vertebra, and in the center of the baſe of the loweſt, we ſhall find a ſmall hole there, which, probably, communicates through the middle of all the vertebræ to the cavity in the center of the head.

Along this ſtem, at different diſtances, from an inch and quarter to a quarter of an inch in length, we obſerve many ſeries of five cylindrical-jointed arms, each ſeries is of equal length, and placed in a wheel or whirl-ſhaped form like the equiſetum or horſetail plant. Each arm is inſerted in one of the five cavities of a vertebra, and each joint into one another; that the upper end of one joint inclines over the lower end of the next to it, which it appears, at the ſame time, to incloſe with a ſmall margin.

Theſe joints are generally about one twelfth of an inch in length, and the ſame in diameter, except a few near their inſertion in the ſtem, which are ſhorter and thicker the nearer they are to it.

We may plainly trace a ſmall hole here through the midſt of the joints, which communicates through the center of the ſtarry vertebræ in the main ſtem, to the hooked joint at the extremity of theſe arms.

On the under or inner ſide of thoſe joints, that are near the end of the arms, we may diſcover four minute tubercles in every joint, two at each end; theſe are of the ſame teſtaceous ſubſtance with the reſt of the joint. By means of this uneven ſurface, together with the hook, which the laſt joint forms, bending downwards,

wards, the animal can take a more ſecure hold of whatever it ſeizes.

But as the ſtem of this animal appears evidently to be broke off ſhort at the bottom, we muſt remain in doubt, whether it moves about in the ſea, or is fixed to rocks and ſhells by a baſe, like corals, ſponges, and keratophytons, until ſome future diſcovery ſhall clear up this matter more to our ſatisfaction.

In examining the main ſtem, or column, we may obſerve ſome ſingle joints or vertebræ projecting a little farther than the reſt. There are generally three or four of theſe in each diviſion, between the whirls of arms; the angular parts of theſe joints end in ſmall round knobs; but the knobs at the corners of the vertebra, immediately under the head of the animal, are remarkably larger than the reſt.

The joints or vertebræ of the ſtem vary in thickneſs, as well as in diameter; the common thickneſs is about one tenth of an inch; but in the laſt four diviſions approaching towards the head, they gradually diminiſh, till they become extremely thin.

We now come to what is called the head, perhaps the body of the animal; for in the center of this dry ſpecimen, there ſtill remains a cup of a cruſtaceous ſubſtance, and of an oval form, about an inch in length, three quarters of an inch over, and a quarter of an inch deep; in the center of this, as was obſerved before, is a ſmall hole, which apparently communicates with the internal part of the vertebræ of the ſtem: in this cup, or cavity, it is probable, were the inteſtines and ſtomach of the animal, as in the aſterias, called caput Meduſæ. This cup is ſupported by the baſes of ſix dichotomous teſtaceous arms, or branches, (perhaps five is the natural number, for one ſeems irregularly placed.) Theſe lower parts, or baſes of the branching arms, conſiſt of three joints each, and ſurround the cup, to which they ſeem united: each of theſe divide into two other jointed branches, that are round or convex on their under ſide, but flattiſh on the upper, with a deep groove running along the middle, which is furniſhed with two rows of ſuckers, as in the ſepiæ and aſteriæ. From the upper edges of each alternate joint of theſe branches, ariſe two rows of ſmall jointed claws, like fingers; theſe two oppoſite rows bend in towards each other: each ſmall branch, or finger, is about half an inch long, and one twentieth of an inch broad; the ſize of theſe joints diminiſh a little, till you come to the laſt joint, which ends in a point. Each of theſe joints is pointed at top, and being concave, embraces the lower convex part of the next above it; theſe are likewiſe furniſhed on their concave ſide with two rows of ſuckers, claſping together; they ſecure their prey with theſe oppoſite claws, or fingers.

As the finer and more ſubdivided branches were broken off, when I received this ſpecimen, I ſhall, in order to give ſome idea of them, lay before the Society drawings from two curious foſſils, belonging to the excellent cabinet of Mr. Francomb. One of them (B) ſhews all the ramified arms of the head cloſed up together, and the other (C) plainly ſhews the ſmall internal claws, or fingers, proceeding from theſe arms. Theſe were found at Pyrton-paſſage in Glouceſterſhire. The foſſils themſelves, with that of the encrinus ſent me from Barbadoes (A), [*Vide Tab.* XIII.] I have now the honour of laying before the Royal Society.

I am,

S I R,

Your moſt obedient ſervant,

Park-Street, Weſtminſter,
Dec. 17, 1761.

John Ellis.

P. S. In order to give a clearer idea of this curious animal, I have added another plate, [*Vide Tab.* XIV.] taken from the French engraving of their encrinus; and, to illuſtrate the plates, I have given a particular deſcription of both of them, with proper references.

The Deſcription of the PLATES.

Plate Nº XIII. repreſents, at

A The exact ſize of the Barbadoes encrinus, or the branched headed ſtarfiſh, with a pentagonous jointed ſtem, having many ranges of cylindrical jointed claws, diſpoſed, at particular diſtances round the ſtem, in form of rays.

B A curious foſſil found at Pyrton-paſſage in Glouceſterſhire, being evidently the head of an encrinus, or ſtarfiſh, of the ſame kind, with all its ſubdivided branches drawn in cloſe together.

C This foſſil, which was found at the ſame place with the former, exhibits part of a branch belonging to the head of the ſame animal, wherein the inward fine jointed fibres, or fingers, exactly agree with the recent ſpecimen.

D A foſſil copied from Roſinus, repreſenting the ſubdiviſion of the branches of the head, with the jointed fibres, or fingers, as in the foregoing.

E A piece of a branch of the head of the Barbadoes encrinus, at F, magnified, to ſhew the diſpoſition of the joints of the fibres, or fingers.

F The mutilated branches of the head of the Barbadoes animal.

G A foſſil aſteria, found in Marſton-truſſel in Northamptonſhire, and copied from Morton's hiſtory of that county, Tab. X. Fig. 19. This plainly appears to be the top of a columnar ſtem, with part of the branches of the head of one of theſe animals.

H Two pieces of the common foſſil aſteriæ, one with its joints united by ſutures, the other plain. This foſſil is well deſcribed by Dr. Liſter, in the Philoſoph. Tranſ. N° 112. p. 274. Tab. II.

I Repreſents one ſingle joint of the foſſil aſteria.

K The cavity at the top of the head, or rather the cavity in the center of the branched arms of the Barbadoes encrinus, where we may reaſonably ſuppoſe the ſtomach and inteſtines were contained.

L The under part of the head, to ſhew the inſertion of the arms.

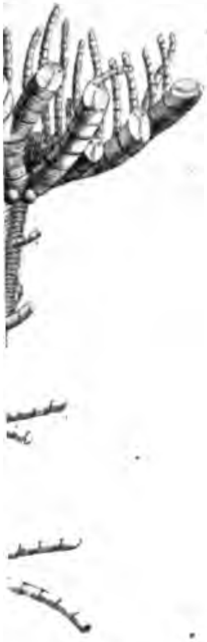
Plate N° XIV. repreſents, at

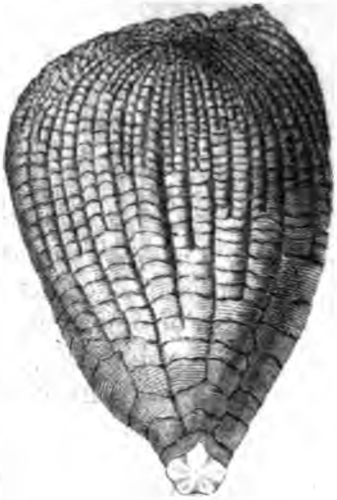
M The Martinico encrinus, or branched headed ſtarfiſh, with a jointed ſtem, ſent to Madam Bois Jourdain, of Paris, by the name of palma marina. This figure is much leſs than the original, which is eighteen inches long.

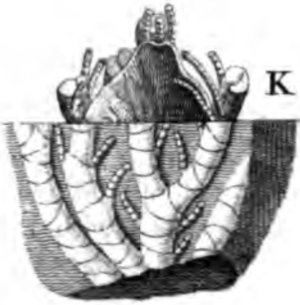
N The under part of the head, with the arms divided in a dichotomous or twofold manner, and diſpoſed like branched rays, each of which is furniſhed with ranges of ſmall fingers, or jointed fibres, placed on each ſide in an alternately pinnated order.

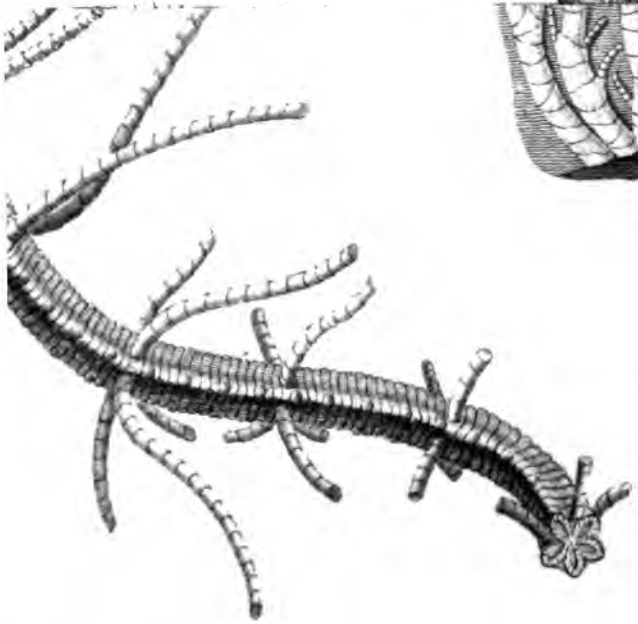
O One of the joints of the main ſtem magnified. In this figure, the five jointed cylindrical claws, which are inſerted in the hollow parts of the vertebra, or joint, are exhibited in different views, as well to diſcover their inward as their outward form and texture. On the upper ſurface of this joint, are moſt elegantly expreſſed thoſe curious indentations, which connect the vertebræ together, con-

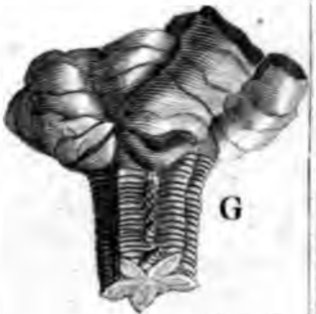
Philos. Trans. Vol. LII. TAB. XIII. *p. 302*











J. Mynde sc.

Q

taining a cartilaginous ſubſtance, that gives ſtrength and pliancy to the animal, to move the main ſtem in any direction.

P The outſide of a part of one of the ſmall arms of the head, with two of the jointed fibres, or fingers, cloſed together.

Q The inſide of the ſame figure.

R This figure expreſſes the ſame part of the animal, but with ſix fingers placed alternately oppoſite; all which, as well as part of the arm, in which they are inſerted, are repreſented expanded, to ſhew the form and diſpoſition of the ſuckers, which are of the ſame kind in this animal, as in the ſepiæ and aſteriæ, or what we call the cuttlefiſh and ſtarfiſh.

S This repreſents four vertebræ of the ſtem, three of which are cut perpendicularly through the middle, to ſhew part of the ſmall tube, which paſſes through the center of all the joints, and to give a view of the uniting of the indentations.

LVII. *Remarks on a Paſſage of the Editor of the* Connoiſſance des Mouvements Céleſtes pour l'Année 1762: *In a Letter to the Right Hon.* George *Earl of* Macclesfield, *Preſident of the Royal Society. By* Matthew Raper, *Eſq;* *F. R. S.*

My Lord,

Read Dec. 17, 1761.

SIR Iſaac Newton, in the ſecond and third editions of his Principia, (lib. iii. prop. 19.) has mentioned Norwood's meaſure of a degree on the meridian, as taken about the year 1635.

The editor of the Connoiſſance des Mouvements Céleſtes pour l'Année 1762, p. 196, has the following paſſage:

" On *pretend* auſſi en Angleterre, que des l'année " 1636, Norwood avoit trouvé le degré par des meſures priſes entre Londres et Yorck de 57300, ou " de 57400 toiſes; réſultat, qui ſe *trouveroit* d'une " exactitude bien ſinguliére pour ce tems la: mais un " fait *plus authentique* c'eſt que Newton en 1666 " jettant les premiers fondemens de ſon admirable " ſyſtème de la gravitation, n'avoit jamais oüi parler " des meſures de Norwood, et ſuppoſoit, avec tous " les pilotes de ſon tems, le degré de 60 milles Anglois, qui font 49200 toiſes."

Here this writer aſſerts, that Sir Iſaac Newton had never heard of Norwood's meaſure in 1666, (of which he can bring no proof) and would thence inſinuate, that, probably, there never was ſuch a one, or at leaſt not ſo early as is pretended. In either caſe, Sir Iſaac, in the propoſition above-mentioned, muſt have poſitively aſſerted what he did not know to be true, or knowingly have publiſhed a falſhood.

Norwood's book is intituled, *The Seaman's Practice; containing a fundamental Problem in Navigation, experimentally verified, namely, touching the Compaſs of the Earth and Sea, and the Quantity of a Degree, in our Engliſh Meaſures, &c. By Richard Norwood, Reader in the Mathematics.* He tells us, that having obſerved the latitude of London in the year 1633, and that of York in 1635, he meaſured the diſtance of the two cities, in his return from York to London; and the account he gives of his meaſurement is ſo clear and ingenuous, that the reader will find no cauſe to doubt either his abilities or his fidelity.

The book was firſt publiſhed in the year 1636, and hath ſince gone through many editions, the eighth being printed in 1668. The title above-mentioned is likewiſe found verbatim in London's catalogue of the moſt vendible books in England, publiſhed in the year 1658, twelve years before Picard meaſured a degree in France; ſo that the authenticity of the fact, that Norwood's meaſure preceded Picard's, cannot be doubted.

The editor of the Connoiſſance, p. 195, 196, has given a liſt of different meaſures of a degree, according to different authors, who had either actually attempted to meaſure one themſelves, or had adopted the meaſure in this liſt for a true one. Among theſe, he has moſt diſingenuouſly put Sir Iſaac Newton's name to a meaſure of ſixty Engliſh ſtatute-miles; which muſt imply, that Sir Iſaac believed this to be neareſt the truth, till he knew of Picard's meaſure in 1670. Whereas it does not appear, nor is it at all probable, that he ever preferred that rude conjectural meaſure to the meaſures of Snellius, and others well known to the learned world before the year 1666; but being at that time retired from Cambridge, on account of the plague, and abſent from his books, having occaſion to uſe the diameter of the earth in a calculation, he took the common account in uſe among ſeamen, as Dr. Pemberton has related, in the preface to his View of Sir Iſaac Newton's Philoſophy. And this anecdote ſeems to be all the authority the French writer had, for aſcribing that meaſure to Sir Iſaac Newton, and for aſſerting, that he had never heard of Norwood's meaſure in the year 1666.

If his view was to do honour to his own country, by depriving others of their due praiſe, the wiſer part of his countrymen will not think themſelves much obliged to him, well knowing, that the reputation of a great kingdom, which has ſo long diſtinguiſhed itſelf in Europe by men eminent in arts and arms, does not ſtand in need of the varniſh of ſuch ungenerous practices.

I am,
My Lord,
With great reſpect,
Your Lordſhip's
moſt obedient ſervant,

Nov. 19, 1761.

Matt. Raper.

LVIII. *An Extract of a Letter of Monſieur* De la Lande, *of the Royal Academy of Sciences at* Paris, *to Dr.* Bevis, *dated there* March 26, 1762. *Tranſlated from the* French.

Read May 13, 1762. I Have received, with a great deal of gratitude, *The Seaman's Practice*, which you were ſo good as to ſend to me. I return you my moſt humble thanks for it. I had never heard, that Norwood's meaſure had been printed ſo early as the year 1636; and I did not think, that before Newton, that is, before 1666, it was at all known. I aſſure you, that I will publiſh in our *Memoires* an extract of this book, in order to do homage to the labours of that celebrated Engliſhman, who had preceded us with relation to the figure of the earth. I am ſorry, that I have ſeemed to have been in doubt when I ſpoke of it, and that my book is already diſperſed: but I ſhall find an opportunity to repair this another time. In the mean time, do me the juſtice to obſerve, that I did not ſay, that Norwood's meaſure did not exiſt; but only, that Newton had no knowledge of it, as ſeems to reſult from the teſtimony of Dr. Pemberton, who relates, that Newton having had the notion of the attraction of earth upon the moon, was diverted from purſuing it, by obſerving, that the earth was too large not to produce a greater attraction. If any member of your illuſtrious Royal Society is offended with my reflections, I deſire you to

make my excuſes to him, and to aſſure the Royal Society of my moſt humble reſpects.

“ J'ai recû avec beaucoup de reconnoiſſance le livre de *Seaman's Practice*, que vous avés eu la bonté de m'envoyer: je vous en fais mes trés humbles remercîmens: je n'avois jamais oüi dire que la meſure de Norwood eut eté imprimée des 1636, et je ne croiois pas qu'avant Newton, c'eſt a dire avant 1666, on en eut en connoiſſance. Je vous aſſure que je ferai imprimer dans nos Memoires un extract de ce livre pour rendre hommage aux travaux du celebre Anglois, qui nous avoit précedé ſur la figure de la terre. Je ſuis faché d'avoir paru en doute lorſque j'en ai parlé, et que mon livre ſoit déja diſtribué par tout; mais je trouverai bien l'occaſion de réparer cela une autre fois. Cependant, faites moi la juſtice d'obſerver, que je n'ai pas dit que la meſure de Norwood ne fut pas exiſtente, mais ſeulement que Newton n'en avoit pas connoiſſance, comme il ſemble reſulter du temoignage de Pemberton, qui raconte, que Newton ayant eu l'idée de l'attraction de la terre ſur la lune, en fut détourné ſur ce que il vit, que la terre etoit trop grande pour ne produire pas une plus grande attraction. Si quelqu'un dans votre illuſtre Societé Royale eſt bleſſée de mes reflexions, je vous prie de lui en faire mes excuſes, et d'aſſurer la Societé Royale de mes tres humbles reſpects.”

LIX. *Ob-*

LIX. *Obſervation of the Tranſit of* Venus *over the Sun,* June 6, 1761, *at the Iſland of* Rodrigues; *by Mr.* Pingré, *of the Royal Academy of Sciences at* Paris. *Tranſlated from the* French, *by* Matthew Maty, *M.D. F. R. S.*

Read April 29, 1762.

JUNE 5th, at about 18h 30′, the Sun roſe amidſt very thick clouds.

At 18h 43′ 51″, Venus was entirely upon the Sun's diſk; the exterior limb of the planet being at the diſtance of at moſt 15″ from that of the Sun. The intervening clouds did not permit me to meaſure the diſtance more exactly. I made uſe of an 18 feet refracting teleſcope. The following obſervations were made with a 9 feet teleſcope of the ſame kind:

True time.			Diſtance of the neareſt limbs.		
h	′	″	′	″	
19	3	54	1	17.1	Haſtily, on account of the clouds.
	12	1	1	29.2	The ſame.
	19	42	2	10.2	Something better.
	25	3	2	35.7	:: Becauſe of clouds.
	29	33	2	38.6	
	34	9	2	50.1	
	41	18	3	5.8	
	56	25	3	41.6	
20	24	46	4	45.4	

True time.			Diſtance of the limb.		
h	′	″	′	″	
20	34	56	5	6.6	
	48	55	5	23.4	
	50	24	5	26.2	* That is to ſay, good.
	54	49	5	31.9	*
21	1	19	5	38.7	High wind.
	12	43	5	47.8	*
	19	17	5	51.2	*
	23	20	5	52.2	*
	25	27	5	52.6	*
	28	35	5	53.0	
	32	35	5	53.4	
	32	56	5	53.4	
	38	13	5	54.6	*
	41	7	5	55.7	** Very good, and the greateſt phaſis of the eclipſe.
	51	16	5	55.3	*
	56	31	5	55.3	
	58	18	5	54.2	
22	1	5	5	51.5	
	5	14	5	45.9	
	10	39	5	40.2	
	18	53	5	32.6	
	25	24	5	25.1	
	32	22	5	13.7	::
	40	4	5	2.4	:: Becauſe of the high wind.
	45	26	4	51.1	
	53	35	4	34.0	
	54	43	4	32.2	

True

True time.			Diſtance of the limbs.		
h	′	″	′	″	
22	56	58	4	28.4	:: High wind.
	59	37	4	24.6	
23	3	13	4	17.0	*
	5	39	4	9.5	*
	7	24	4	1.9	::
	11	6	3	54.4	
	15	6	3	46.8	*
	18	40	3	37.35	
	23	7	3	27.9	::
	25	44	3	18.45	High wind.
	29	20	3	9.0	
	32	54	2	59.55	High wind.
	36	57	2	50.1	
	40	33	2	40.65	
	43	54	2	31.2	
	47	9	2	21.75	Wind.
	50	55	2	12.3	
	54	14	2	2.85	Wind.
	57	20	1	53.4	Wind.
0	0	17	1	43.95	Gentler wind.
	4	34	1	34.5	Wind.
	7	37	1	25.05	
	11	16	1	15.6	*
	14	28	1	6.15	Wind.
	17	49	0	56.7	

The diameter of Venus, meaſured ſeveral times, 54″½.

The following obſervations were made with the 18 feet teleſcope:

h	′	″	
0	34	47	The contact of the occidental limbs.
	52	5	Venus, almoſt got off, is covered with a cloud.
	52	23	It is ſtill ſeen, but little; another cloud.
	53	18	The Sun's diſk ſeems ſtill a little altered; but this perception is faint, and a new cloud prevents my making a better obſervation.
	54	21	The tranſit is certainly ended.

I found the latitude of my obſervatory to be 19° 40′ 40″ ſouth.

As for the longitude, the following obſervations are the only ones, which the ſky, almoſt conſtantly cloudy, in the night-time, permitted me to make, amidſt the tumult of arms.

June 9. An immerſion of α in ♍, under the dark diſk of the Moon, $8^{h}\ 46'\ 24''$. The immerſion certainly did not come ſooner; but, on account of a light cloud, which then paſſed over the Moon, it might have happened a little later. The doubt, however, cannot extend beyond 2″ of time. The ſtar paſſed to the north of the Moon's center. I could not ſee the emerſion, becauſe of the clouds. I made uſe of the 9 feet teleſcope.

June 21. A very uncertain obſervation of an immerſion of ε in ♌, at $9^{h}\ 39'\ 16''\frac{1}{2}$. I thought to have had before a ſight of the ſtar; but dare not affirm it. At $10^{h}\ 10'\ 45''$, the emerſion was certainly obſerved, with the ſame teleſcope.

June 22, at $14^{h}\ 48'\ 55''$. Immerſion of the firſt ſatellite of ♃. A good obſervation, with the 18 feet teleſcope.

July 16, at $14^{h}\ 1'\ 23''\frac{1}{4}$, the Moon and σ ♐ get together under a light cloud, and I loſe ſight of the ſtar. I believe this to be the true time of the immerſion. It is at leaſt certain, that, after 5 or $6''$, the cloud being diſperſed, the ſtar was covered under the ſouth part of the Moon. I made uſe of a 5 feet teleſcope. The clouds prevented my obſerving the emerſion, as likewiſe the eclipſe of the firſt ſatellite of ♃.

July 31, at $12^{h}\ 16'\ 7''$, an immerſion of the ſecond ſatellite. A very dubious obſervation.

The ſame day, at $13^{h}\ 10'\ 29''$. Immerſion of the firſt ſatellite. A good obſervation, with the 18 feet teleſcope.

September 1. Immerſion of the firſt ſatellite, at $9^{h}\ 49'\ 40''$. Doubtful to a few ſeconds, on account of the clouds; with the ſame teleſcope.

The variation of the magnetical needle is 10° 42′ N. W.

In my obſervation of Venus, I found its diameter much ſmaller than I expected. I am ſhort-ſighted; ſome light clouds, which now and then paſſed over the Sun, had obliged me to uſe only a ſlightly ſmoked glaſs; and laſtly, the objective-glaſs of my 9 feet teleſcope does not appear to me ſufficiently perfect. I could not meaſure with the micrometer the diameter of the Sun, which ſomewhat exceeded that of the field of my teleſcope. The 18 feet teleſcope, which I uſed in my obſervation of the egreſs, is excellent.

I don't

I don't doubt, that, if I could have adapted the micrometer to it, the diameter of Venus would have appeared larger than it did with the 9 feet teleſcope. I believe, that, by adding the ſemidiameter of Venus, as I obſerved it, to the greateſt phaſis $5' \, 55''.7$, deducting from the ſum one half of the exceſs of the true diameter of Venus above the obſerved one $54' \frac{1}{2}$, and laſtly, by ſubſtracting the remainder from the ſemidiameter of the Sun, the leaſt diſtance of the centers will be found pretty exactly; which was one of the principal obſervations I propoſed to make.

My obſervations of the diſtances of the limbs, or at leaſt the greateſt number of them, cannot be depended upon, to more than one ſecond. In this almoſt uninhabited iſland, I wanted ſeveral conveniencies; and, notwithſtanding all my endeavours, the high wind often diſordered my inſtrument.

The method I made uſe of, appeared to me to be the beſt for the determination of the parallaxes of the Sun and Venus. I knew that this phænomenon would be obſerved elſewhere, by methods, that would more directly determine the moſt important elements of the orbit of Vénus.

I have ſeen no ſatellite of this planet; nor was Mr. Thuillier, profeſſor of mathematics, and appointed to aſſiſt me, by the King and the Academy, luckier than myſelf.

I preſent theſe obſervations to the Royal Society of London, as a juſt tribute of my eſteem, my reſpect, and my gratitude, being ſenſible that I owe to ſome of the members the paſſport, which the Engliſh admiralty were graciouſly pleaſed to give me.

I have already made uſe of ſome other opportunities, to convey my obſervation into England; but not knowing whether it was received, I ſend this preſent copy, which is both more copious, and ſomewhat more correct.

Pingré,

Of the Royal Academy of
Sciences of Paris.

Liſbon,
March 6, 1762.

LX. *Obſervations made at the Cape of* Good Hope; *by Mr.* Charles Maſon *and Mr.* Dixon; *reduced to apparent Time by Mr.* Maſon. *With an Appendix.*

Read April 22, 1762.

TABLE for the object-glaſs micrometer, applied to the object-end of the tube of a reflecting teleſcope of two feet focal length, its focal length being 495.48 inches.

Inches.	Angle.	Decimals of an inch.	Angle.	Diviſions of Vernier.	Angle.
	′ ″		′ ″		″
1	7 5.9	.05	0 21.3	1	0.852
2	14 11.9	.10	0 42.6	2	1.7
3	21 17.8	.15	1 3.9	3	2.6
4	28 23.8	.20	1 25.2	4	3.4
5	35 29.7	.25	1 46.5	5	4.3
		.30	2 7.8	6	5.1
		.35	2 29.1	7	6.0
		.40	2 50.4	8	6.8
		.45	3 11.7	9	7.7
		.50	3 33.0	10	8.5
		.55	3 54.3	11	9.4
		.60	4 15.6	12	10.2
		.65	4 36.9	13	11.1
		.70	4 58.2	14	11.9
		.75	5 19.5	15	12.8
		.80	5 40.8	16	13.6
		.85	6 2.0	17	14.5
		.90	6 23.3	18	15.3
		.95	6 44.6	19	16.2
		1.00	7 5.9	20	17.0
				21	17.9
				22	18.7
				23	19.6
				24	20.4
				25	21.3

MEMORANDA.

The body of the obſervatory (erected at the Cape) was circular, the radius of which 6½ feet in the clear; the height of the circular wall 5½ feet; the roof conical, and moveable, (made of board) a lid in it of 3 feet breadth, to open, which was eaſily turned to any part of the heavens, as the whole top moved freely.

The clock was fixed againſt two pieces of timber (let near 4 feet into the ground) of 10 inches by 8; theſe pieces being joined together by pins of 1½ inch diameter.

The mean of Farenheit's thermometer, as it ſtood at 6 or 7 in the morning, noon, 1 or 2^h after, and 7 or 8 in the evening.

From 27th of May 1761 to June 10th	59.5,	Extremes	53 to 65.
From 10th of June to 20th - - -	59.2,	Ditto -	50 to 67.
From 20th of June to 30th - - -	57.9,	Ditto -	51 to 68.
From 1ſt of July to 15th - - -	56.3,	Ditto -	50 to 65.
From 15th of July to 30th - - -	54.3,	Ditto -	47 to 60.
From 1ſt of Auguſt to 15th - - -	56.9,	Ditto -	48 to 66.
From 15th of Auguſt to 30th - -	56.0,	Ditto -	48 to 68.
From 1ſt of September to 15th - -	57.4,	Ditto -	50 to 69.
From 15th of September to 26th -	54.8,	Ditto -	49 to 64.

At 47 upon 18th July, in the morning, and 69 the 1ſt and 14th of September, in the afternoon. Theſe were the greateſt differences I ſaw.

June 6th, at the end of the tranſit, 55.

Note. Thoſe obſervations marked : are a little dubious.
Theſe marked :: are very dubious.
The tranſit was obſerved with the power that magnified 120 time.
The eclipſes of the ſatellites of Jupiter with the ſame power.

The adjuſtment of the nonius of the micrometer, thus - - -

— 0 0 2
+0 1
0 0
—0 3 —
+0 0 +
0 0
0 0
—0 0½
0 0
0 0
—0 2 —

Mean = — 0 0.52 = 0.4 to be added to the obſerved angle, it falling to the left hand of 0.

Obſervations made at the Cape of Good Hope.

1761.
April
☽ 27^d. Arrived in Sable bay. - - - - - -
May
♄ 2. Carried the inſtruments aſhore.
☽ 4. Set the clock going, the pendulum having not been altered ſince it came from London. -

The quadrant being fixed, the plumb-line ſhewing it did not move.

Stars paſſed the wires per clock.	The ſtars returned to the ſame plane.	Going of the clock. Loſes per day of the ſtars.
Procyon. ☽ 4^d May.	Procyon. ♂ 5th.	
h ′ ″	h ′ ″	′ ″
2 12 5½	2 9 48½	2 17
14 36	12 18	2 18
17 7½	14 51	2 16½
Procyon. ♂ 5th.	Procyon. ☿ 6th.	
2 33 47 :	2 31 31	
36 26	34 9 —	
39 7½	36 48½	

Farenheit's thermometer.

May	7^h A. M.	2 or 3^h P.M.	7^h P.M.
10		65	
11		62	
13	56	60	
14	55	59	57
15	55	60	59
17		62	63
18	58	61	
19	58	63	64
20	60	65	
21	60	64	65
22	60½	63	62½
23	58	61½	
24	58	60½	
25	58	63	63½
26	59	62½	63

1761.	Stars paſſed the wires per clock.	The ſtars returned to the ſame plane.	
May.	Caſtor. ☉ 10th.	Caſtor. ♂ 12th.	
	h ′ ″	h ′ ″	
	2 55 28	Clouds.	
	Clouds.	2 49 30 +	
	56 26½	51 52	
	57 6½	52 32	
	Pollux. 12th.	Pollux. 13th.	
	3 2 27	3 0 11½	
	4 17	2 0	
	6 5½	3 49	
	7 15½	4 57½	
	Pollux. 13th.	Pollux. 14th.	
Quadrant as before, and took the other wires, which clouds prevented, on the 12th.	3 0 11½	3 9 0	
	2 0	59 43 —	
	3 49	1 33 —	
	4 57½	2 41½	
	7 29½	5 9 +	
	8 0	5 43	
	Regulus. 16th.	Regulus. 17th.	
	3 33 23½	3 31 6	
	34 42	32 25½	
	35 39 +	33 21	
	37 56	35 38 —	
	Time per clock.		
May	h ′ ″	° ′ ″	
☽ 18d.	2 11 24 —	55 34 28	Procyon apparent or obſerved zenith diſtances.
	19 52	56 59 0	
	25 26½	57 56 2	
	Procyon. 18d.	Procy. 19d returned.	
	2 34 31½	2 32 17 —	
	35 1½	32 43½	
	37 26	35 11	
	37 52 —	35 34½	
	2 38 42½	36 37 —	

1761.	Time per clock. h	′	″			Apparent time. h	′	″
May ☽ 18d.	5	24	0	Cloudy.				
	5	36	0	The eclipſe of the ☽ had been begun ſome time.				
	6	36	5	Entrance into total darkneſs -	all very clear	10	53	28
	7	58	20	Emerſion - - - - - -		12	15	37
	9	6	30	The end of the eclipſe - - -		13	23	42

Hitherto the clock ſtood on a lower floor, near to the place intended for the obſervatory; and the obſervatory being now finiſhed, I put the clock into it, wound up the pendulum, and ſet it to nearly ſyderial time.

No obſervations were made material to June 5th, it being cloudy near all the time; but the 5th, in the evening, it fortunately cleared up.

	h	′	″			°	′	″	
June ♀ 5d.	11	39	21	+	obſerved zenith diſtances	59	25	0	Antares. I ſet the quadrant to theſe even minutes, and then waited for the ſtar paſſing of the wire.
	11	55	16½			56	11	0	
		58	12	+		55	35	0	
	12	11	44			52	49	0	
	Clouds.								

Equal altitudes. East. Time per clock. h ′ ″	Equal altitudes. West. Time per clock. h ′ ″	Paſſed the meridian. Time per clock. h ′ ″		°	′	″
12 19 45	20 8 53½	16 16 16¼	Antares point	50	40	0
22 12½	10 21 +					
Clouds.	Clouds.					
12 52 4½	19 35 37 +	16 16 18	Ditto - -	44	0	0
Clouds.	37 4½					
56 59	40 30	16 16 17¼				
13 4 58 −	19 22 44		Ditto - -	41	20	0
7 24 +	25 11 +	16 16 17½				
Clouds.	27 37½	16 16 17¼				
13 16 13 +	19 11 29 −	16 16 18	Ditto - -	39	0	0
18 39	13 55½	16 16 17¼				
21 7 −	16 22 :	16 16 17½				

1761.	Equal altitudes. East. Time per clock.	Equal altitudes. West. Time per clock.	Paſſed the meridian. Time per clock.			
	h ′ ″	h ′ ″	h ′ ″		° ′ ″	
June ♀ 5^{d}.	16 9 39	23 5 54 −	19 40 37¼	α Aquilæ	64 48 20	Mr. Dixon.
	12 29½	8 45 :	19 40 37¼			
	15 20½	11 34	19 40 36½			
	21 44	22 53 46 :	19 40 36½	Ditto	62 40 0	
	24 37	56 37	19 40 37			
	27 27	59 31	19 40 37½			
	16 33 16 +	22 41 58	19 40 37	Ditto	60 40 0	
	36 15½	44 59 −	19 40 37¼			
	39 16	47 58	19 40 37			

	Time per clock.	Tranſit of Venus.
	h ′ ″	
June ♀ 5^{d}.	0 12 0	The ☉ aſcended in a thick haze, and immediately entered a dark cloud.
	0 35 0	The firſt ſight of the planet.
	0 48 40	Very hazy.
	0 52 0	Cloudy.
	1 0 0	Ditto.

Time per clock.	Parts of the micrometer. Inches.		
1 18 7	3 90 5	The ☉'s fartheſt limb from ♀'s fartheſt limb. That is, the ☉'s northern limb from ♀'s ſouthern limb.	
27 18	3 95 5 +	Ditto.	
30 4	3 95 15	Ditto.	
33 5	4 0 0	Ditto.	
35 15	4 0 4	Ditto.	
37 40	4 0 17 ::	Ditto.	
39 0	4 40 23 −	The ☉'s diameter.	By a mean of theſe four obſervations, the ☉'s diameter is = 31′ 33″.3.
	4 40 20½	Ditto.	
44 0	4 40 21 +	Ditto.	
46 0	4 40 23½	Ditto.	

1761.	Time per clock.	Parts of the micrometer.		Apparent time.
June	h ′ ″	Inches.		h ′ ″
♀ 5^d.	1 48 20	4 10 0 −	The ☉'s fartheſt limb from ♀'s fartheſt limb, as before.	
	50 58	4 10 5	Ditto.	
	55 30	0 10 19½	Venus's diameter. } By a mean of theſe three obſervations, the diameter of Venus is = 59″.6.	
	57 0	0 10 20 −	Ditto.	
	59 0	0 10 19 +	Ditto.	
	2 2 23	4 19 19 −	The ☉'s fartheſt limb from ♀'s, as before.	
	3 55	4 19 22½	Ditto.	
	5 45	4 20 5 +	Ditto.	
	2 39 16		The time of internal contact } very clear	21 39 52 —
	56 50		Ditto external - - - - } very clear	21 57 23 —
	2 39 12		} Ditto, per Mr. Dixon.	
	56 48			
	3 10 0		Cloudy.	
	9 5 0		Saw the ☉ (but no ſatellite). Cloudy after, till night.	

N. B. The adjuſtment of the nonius of the micrometer as upon the 17th May.

When I ſaw the planet firſt, its periphery, and that of the Sun's, were in a great tremour; but this vaniſhed, as the Sun roſe, and became well defined.

Four minutes before the internal contact, the Sun's diſk was entirely hid by a cloud, for about one minute.

	Equal altitudes.		Paſſed the mer.					
	Time per clock.	Time per clock.	Time per clock.					
	h ′ ″	h ′ ″	h ′ ″		°	′	″	
☿ 10.	12 51 39½	19 35 11	16 15 51½					Mr. Dixon.
	54 5½	37 35	16 15 50½	Antares	44	0	0	
	56 32½	Clouds.						
	13 4 32	19 22 17						
	6 59	24 44	16 15 51½	Ditto	42	20	0	
	Clouds.	27 10 +	16 15 51					

1761.	Altitude. Time per clock.		Apparent time.
	h ′ ″		h ′ ″
June ♃ 11.	20 13 55	The ſecond ſatellite of ♃ immerged. Foggy air. The ſatellites appeared faint.	14 51 24

	Equal altitudes. Time per clock.	Equal altitudes. Time per clock.	Paſſed the meridian. Time per clock.			
	h ′ ″	h ′ ″	h ′ ″		° ′ ″	
♀ 12.	12 19 10½	20 7 20½	16 15 43	Antares	50 40 0	Mr. Dixon.
	21 37½	9 50 −	16 15 43¾			
	24 5½	12 18	16 15 44¼			
	12 35 21	19 51 10 +	16 15 43	Ditto -	47 20 0	
	37 48	53 37	16 15 42½			
	40 16	56 6 −	16 15 43½			
	19 33 40	The * ϰ Virginis immerged behind the ☽. The occultation was at the ☽'s northern limb, near the interſection of light and darkneſs.			Apparent time. h ′ ″ 14 7 12	
					° ″	
☉ 14.	12 19 1	20 7 10	16 15 33½	Antares	50 40	Mr. Dixon.
	21 28½	9 38 +	16 15 33¼			
	23 57	12 6½	16 15 33¾			
					° ′ ″	
☽ 15.	12 35 6½	19 50 55 +	16 15 28½	Ditto - -	47 20 0	
	37 34	53 23	16 15 28½			
	40 1½	55 50 +	16 15 28¾			
			° ′ ″			
	22 45 6 ::	Zenith diſtance	3 2 6	Fomalhaut upon the meridian. Plane of the quadrant facing the weſt.		
			h ′ ″		° ′ ″	
♂ 16.	12 35 0½	19 50 50	16 15 22¾	Antares -	47 20 0	
	37 28 −	53 18	16 15 23			
	39 55½	55 44½				
	21 9 30	The third ſatellite of ♃ emerged - - - -			Apparent time. h ′ ″ 15 26 32	

1761.	Altitude. Time per clock.			
	h ′ ″		° ′ ″	
June ♂ 16.	22 44 55	Zenith diſtance	3 3 45	Fomalhaut upon the meridian. Plane of the quadrant facing the eaſt.

By the obſervations of Fomalhaut, it appears the quadrant does not ſhew the true angle, I new-adjuſted it, &c. &c.

	Equal altitudes. Time per clock.	Equal altitudes. Time per clock.	Paſſed the meridian. Time per clock.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
♃ 18.	16 53 49½	22 18 44½	19 39 28¼	α Aquilæ -	57 5 0
	57 0 —	21 56½	13 39 28¼		
	17 0 12	25 7 —			
			° ′ ″		
	22 44 40	Zenith diſtance	3 2 28	Fomalhaut upon the meridian. Plane of the quadrant facing the eaſt.	

			Apparent time. h ′ ″
	23 18 35	The ſecond ſatellite of ♃ immerged - - -	17 27 11

	Equal altitudes.	Equal altitudes.	Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
♀ 19.	12 18 34 —	20 6 36½	16 15 3¼	Antares - - -	50 40 0
	21 1½	9 5½	16 15 3½		
	23 30 —	11 33 +	16 15 3½		
			° ′ ″		
	14 50 24½	Zenith diſtance	9 35 40	γ Scorpii upon the merid.	
	15 44 45½	Ditto - -	8 31 32	π.	
	16 15 8 —	Ditto - -	8 2 56	Antares.	
	17 7 40½	Ditto - -	9 11 38	θ Ophiuchi.	
	18 40 50 —	Ditto - -	7 21 34	σ Sagittarii.	

			Apparent time. h ′ ″
♄ 20.	23 21 54	The firſt ſatellite of ♃ immerged - - - -	17 22 26

	Equal altitudes.	Equal altitudes.	Paſſed the mer.	
	h ′ ″	h ′ ″	h ′ ″	
☉ 21.	2 22 54 —	9 31 47½	6 0 35¼	Sun's limbs. Mr. Dixon.
	26 5 +	35 4½	6 0 35¾	
	29 24 +	38 17	6 0 35½	

1761.

1761.	Altitude. Time per clock.			
	h ′ ″		° ′ ″	
June ☉ 21.	13 12 46 :	Zenith diſtance	20 0 55	Spica upon the meridian.
	Equal altitudes.		Paſſed the meridian.	
	Time per clock.	Time per clock.	Time per clock.	
	h ′ ″	h ′ ″	h ′ ″	° ′ ″
	15 52 35 :	23 20 14	19 39 10½	α Aquilæ - 67 40 0
	55 21½	22 59 +	19 39 10¼	
	58 7 —	25 45½	19 39 10¼	
☽ 22.	2 14 25½	19 48 36 +	6 4 39 +	Sun's limbs. Mr. Dixon.
	17 33½	51 46½	6 4 40	
	20 42½	54 55	6 4 40	
	26 57½	35 51½	6 4 39	Ditto.
	30 9½	39 7 +	6 4 38¾	
	33 26½	42 21 +	6 4 39½	
			° ′ ″	
	13 12 35 ::	Zenith diſtance	24 1 2	Spica upon the meridian.
	14 4 54	Ditto - -	54 20 45	Arcturus, ditto.
	34 0	Ditto - -	61 59 52	ε Bootis.
	15 24 0	Ditto - -	61 26 10	α Coronæ borealis.
♂ 23.	13 12 37 ::	Ditto - -	24 1 8	Spica upon the meridian.
	14 4 44	Ditto - -	54 20 42	Arcturus, ditto.
	34 0	Ditto - -	61 59 48	ε Bootis, ditto.
	16 14 0	Ditto - -	8 3 3	Antares, ditto.
	22 33 30	The third ſatellite of ♃ not immerged.		
	Clouds.	Clouds.		
	36 6	It was immerged.		
			° ′ ″	
	22 44 0	Zenith diſtance	3 3 30	Fomalhaut upon the meridian. Quadrant weſt.

From this day to the 1ſt of July, cloudy, with ſtrong winds and rain.

1761.	Equal altitudes.		Passed the mer.		
	Time per clock.	Time per clock.	Time per clock.		
July	h ' "	h ' "	h ' "		° ' "
☿ 1.	12 58 24½	19 25 46 :	16 14 32 :	Antares -	42 20 0
	13 0 51 +	28 14 :	16 14 32½		
	3 18	Clouds.			
	14 29½	19 0 0		Ditto - -	39 0 0
	16 56	12 8½	16 14 32¼		
	19 23 +	14 35	16 14 32¼		
			° ' "		
	17 17 23	Zenith diſtance	2 58 6 S.	λ Scorpii upon the meridian. Plane of the quadrant weſt.	
♃ 2.	16 35 27	Ditto - -	3 40 48 S.	μ Scorpii upon the meridian.	Plane of the quadrant facing the weſt.
	17 17 0	Ditto - -	2 58 8 S.	λ Ditto.	
	18 5 0	Ditto - -	4 1 45 N.	♂ Sagittarii.	
	22 44 0	Ditto - -	3 3 8 N.	Fomalhaut.	
♀ 3.	16 35 30	Ditto - -	3 40 52 S.	μ Scorpii upon the meridian.	Plane of the quadrant facing the weſt.
	17 0	Ditto - -	2 58 0 S.	λ Ditto.	
	18 0 0	Ditto - -	4 1 42 N.	♂ Sagittarii.	
	22 44 0	Ditto - -	3 3 4 N.	Fomalhaut.	
	Equal altitudes.		Paſſed the mer.		
	h ' "	h ' "	h ' "		° ' "
♂ 7.	13 14 23	19 9 33	16 14 24½	Antares point	39 0 0
	16 49	12 0 +	16 14 24½		
	19 16 +	14 26½	16 14 24½		
			° ' "		
	16 35 24	Zenith diſtance	3 41 32 : S.	μ Scorpii upon the meridian.	Plane of the quadrant facing the eaſt.
	17 17 0	Ditto - -	2 58 40 S.	λ Ditto.	
	18 5 0	Ditto - -	4 1 0 N.	♂ Sagittarii.	
☿ 8.	16 35 19	Ditto - -	3 41 34 S.	μ Scorpii upon the meridian.	Plane of the quadrant facing the eaſt.
	17 17 0	Ditto - -	2 58 48 S.	λ Ditto.	
	18 5 0	Ditto - -	4 1 8 N.	♂ Sagittarii.	

1761.	Equal altitudes.		Paſſed the meridian.		
	Time per clock.	Time per clock.	Time per clock.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
July ♃ 9.	13 14 13	19 9 22½	16 14 14½	Antares - -	39 0 0
	16 39 −	11 49 +	16 14 14		
	19 6½	14 16 −	16 14 14½		
			° ′ ″		
	16 35 8	Zenith diſtance	3 41 28	μ Scorpii upon the meridian.	Plane of the quadrant facing the eaſt.
	17 17 0	Ditto - -	2 58 46	λ Ditto.	
	18 5 0	Ditto - -	4 1 10 :	δ Sagittarii.	
	22 44 0	Ditto - -	3 2 33	Fomalhaut.	
	Equal altitudes.		Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
☉ 12.	13 31 39 −	18 51 25	16 13 59	Antares - -	35 20 0
	34 5	53 53 +	16 13 59		
	36 33 −	56 19½	16 13 59½		
			° ′ ″		
	15 50 45 +	Zenith diſtance	14 47 22	σ Scorpii upon the meridian. Plane of the quadrant facing the weſt.	
	Equal altitudes.		Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
☿ 15.	13 0 0	18 51 10½	16 13 43¾	Antares - -	35 20 0
	33 49	53 38	16 13 43½		
	36 17 −	56 5			
	17 18 30	σ Sagittarii made a near appulſe to the ☽ 's limb. The eye could not diſcover by the teleſcope, that it altered its diſtance, till about 17h 35′.			
		Nonius of the micrometer. Inches.			
	17 45 18	0 15 15½	Moon's ſouthern limb from σ Sagittarii.		
	52 55	0 30 3	Ditto.		
	57 58	0 40 5½	Ditto.		
	18 2 15	0 50 11½	Ditto.		
	7 45	0 65 6 + :	Ditto. A little hazy.		

1761.	Equal altitudes. Time per clock.	Equal altitudes. Time per clock.	Passed the meridian. Time per clock.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
July ♀ 17.	16 0 22 +	23 9 47½	19 37 53½	α Aquilæ -	66 0 0
	3 10	12 36	19 37 53		
	5 59½	15 24	19 37 53		
					Apparent time. h ′ ″
☽ 20.	1 10 35	The ſecond ſatellite of ♃ immerged - - - -			17 9 42
	Equal altitudes.		Paſſed the mer.		
	° ′ ″	h ′ ″	h ′ ″		° ′ ″
♂ 21.	5 14 59½	10 41 53 −	8 2 5	Sun's limb -	66 30 0
	18 38 −	45 32 +	8 2 5		
	22 17 +	49 11 −	8 2 5½		
☿ 29.	15 20 19½	23 48 32 +	19 37 5¼	α Aquilæ -	73 20 0
	22 59	51 11½	19 37 5½		
	25 38½	53 51 :			
					Apparent time. h ′ ″
	20 59 1	The third ſatellite of ♃ immerged - - -			12 24 0
	23 52 42	Ditto emerged - - - - - - - - - - -			15 17 13
	0 18 15	The firſt ſatellite immerged - - - - - -			15 42 42
	0 18 17	Ditto, per Mr. Dixon.			
	Equal altitudes.		Paſſed the mer.		
Aug.	h ′ ″	h ′ ″	h ′ ″		° ′ ″
♀ 7.	15 30 13½	23 37 18 −	19 36 28	α Aquilæ -	71 20 0
	32 54 −	39 59 +	19 36 26½		
	35 36	42 40½	19 36 27		
					Apparent time. h ′ ″
	20 52 32	The 2d ſatellite of ♃ immerged. A litle hazy			11 43 26
	21 14 55	The firſt immerged. Clear - - - - -			12 5 46
			° ′ ″		
♄ 8.	15 48 54 −	Zenith diſtance	14 47 40	σ Scorpii upon the meridian.	
	16 12 0	Ditto - -	8 3 0	Antares ditto.	
☉ 9.	17 4 45	Ditto - -	9 11 42	θ Ophiuchi upon the meridian.	
	18 3 0	Ditto - -	4 1 28	δ Sagittarii ditto.	

1761.

1761.	Equal altitudes.		Paſſed the meridian.		
	Time per clock.	Time per clock.	Time per clock.		
Aug.	h ′ ″	h ′ ″	h ′ ″		° ′ ″
☽ 10.	15 19 28 +	23 47 37	19 36 12	α Aquilæ -	73 20 0
	22 7 −	50 17 +	19 36 12		
	24 47 −	52 55½	19 36 11¾		
			° ′ ″		
	16 3 50 -	Zenith diſtance	8 55 48	σ Scorpii upon the meridian.	
	11 0	Ditto - -	8 2 54	Antares ditto.	
					Apparent time. h ′ ″
	0 53 45	The fourth ſatellite of ♃ immerged - - -			15 32 57
	Equal altitudes.		Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
☿ 12.	15 19 18½	23 47 28	19 36 2¾	α Aquilæ -	73 20 0
	21 58 :	50 8 −	19 36 3 :		
	24 37½	52 47 −	19 36 2¾		
					Apparent time. h ′ ″
14.	23 36 11	The firſt ſatellite of ♃ immerged - - - - -			14 0 50
♃ 20.	I put the clock forward.				
	Equal altitudes.		Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
♀ 21.	16 39 20½	22 36 0½	19 40 43¾	α Aquilæ -	59 40 0
	42 22	39 4½	19 40 43¾		
	45 26 −	42 6½	19 40 43½		
					Apparent time. h ′ ″
	2 2 35	The firſt ſatellite of ♃ immerged - - - -			15 56 2
	2 30	Ditto, per Mr. Dixon.			
	Equal altitudes.		Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
☉ 23.	16 51 19 :	22 23 34 −	19 40 35¾	α Aquilæ -	57 40 0
	54 27 −	26 44½	19 40 35¾		
	57 37½	29 52 +	19 40 35½ :		
					Apparent time. h ′ ″
	20 38 8	The firſt ſatellite of ♃ immerged - - -			10 25 10
	38 2	Ditto, per Mr. Dixon.			

1761.

1761.	Equal altitudes. Time per clock. h ′ ″	Equal altitudes. Time per clock. h ′ ″	Passed the meridian. Time per clock. h ′ ″			
Aug. ♃ 27.	16 50 58 +	22 23 14 −	19 40 15		° ′ ″	
	54 6 −	26 24	19 40 15	α Aquilæ -	57 40 0	
	57 16 +	29 32 +	19 40 15 +			
					Apparent time. h ′ ″	
	20 20 44	The fourth ſatellite of ♃ immerged - - -			9 53 32	
	20 40	Ditto, per Mr. Dixon.				
	23 16 38	The fourth ſatellite emerged - - - - -			12 49 0	
	Equal altitudes. h ′ ″	h ′ ″	Paſſed the mer. h ′ ″			
☉ 30.	16 50 40½	22 22 58 −	19 39 58¼		° ′ ″	
	53 49 −	26 8 −	19 39 58¼	α Aquilæ -	57 40 0	
	56 59 −	29 16	19 39 58¼			
					Apparent time. h ′ ″	
	22 59 45	The firſt ſatellite of ♃ immerged - - - -			12 21 32	
Sept. ♂ 1.	19 47 10 :	The 2d ſatellite of ♃ immerged. Flying clouds			9 2 20	
	Equal altitudes. h ′ ″	h ′ ″	Paſſed the mer. h ′ ″			
♃ 3.	17 22 52½	21 49 16	19 39 42¼ ::		° ′ ″	
	26 24 +	52 52	19 39 38	α Aquilæ -	52 40 0	
	30 8½ ::	56 25½	19 39 39			
					Apparent time. h ′ ″	
	17 53 30	A ſmall ✱ immerged behind the ☽			7 1 52	Mr. Dixon.
	18 26 7	Another very ſmall ✱ ditto - -			7 34 24	
	19 28 22	The 3d ſatellite of ♃ immerged -			8 36 33	
	28 25	Ditto.				
	Equal altitudes. h ′ ″	h ′ ″	Paſſed the mer. h ′ ″			
♀ 4.	17 Clouds.	21 49 10	19 39 32¼		° ′ ″	
	26 18½	52 47½	19 39 33	α Aquilæ -	52 40 0	
	29 55½	56 19 +				
					Apparent time. h ′ ″	
	18 2 4	ϰ Libræ immerged behind the ☽ - - -			7 6 53½	
	19 23 0 ::	Ditto. emerged from the ☽.				

1761.

1761.	Equal altitudes.		Paſſed the meridian.		
	Time per clock.	Time per clock.	Time per clock.		
Sept.	h ′ ″	h ′ ″	h ′ ″		° ′ ″
☉ 6.	17 22 32 −	21 48 56½	19 39 19½	α Aquilæ -	52 40 0
	26 5½	52 32½	19 39 19		
	29 42½	56 6 −	19 39 19		
			° ′ ″		
☽ 7.	18 13 24	Zenith diſtance	8 23 46	λ Sagittarii upon the meridian.	
					Apparent time. h ′ ″
♂ 8.	22 52 15	The ſecond ſatellite of ♃ immerged - - -			11 42 20
	Equal altitudes.		Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		h ′ ″
♂ 22.	19 0 29 +	2 20 47½	22 43 8¾	Fomalhaut -	46 0 0
	3 0	23 18 −	22 43 9		
	5 30 −	Clouds.			
			° ′ ″		
	19 37 48	Zenith diſtance	42 10 42	α Aquilæ upon the meridian.	
	22 43 16½	Ditto - -	3 3 16	Fomalhaut ditto.	
					Apparent time. h ′ ″
♃ 24.	21 27 14	The firſt ſatellite of ♃ emerged - - - -			9 21 35
	27 15	Ditto, per Mr. Dixon.			
	Equal altitudes.		Paſſed the mer.		
	h ′ ″	h ′ ″	h ′ ″		° ′ ″
☉ 27.	19 29 38½	1 51 1	22 42 48½	Fomalhaut -	40 0 0
	32 7 +	53 29½	22 42 48¼		
	34 36 −	55 57½	22 42 48		
☽ 28.	Packed up the inſtruments.				
♂ 29.	Put them on board the Mercury, Capt. Harrold.				
Oct.					
♄ 3.	Sailed for St. Helena.				

Charles Maſon.

The inſtruments made uſe of, in theſe obſervations, were,

Two reflecting teleſcopes, each two feet focal length, and magnifying 120 times, made by Mr. Short.

A quadrant of one foot radius, made by Mr. Bird, and the property of the Earl of Macclesfield.

An aſtronomical clock, made by Mr. Ellicott.

A P P E N D I X.

Eclipſes of Jupiter's ſatellites, obſerved at the Royal Obſervatory at Greenwich, with a reflector of two feet focus, magnifying 95 times.

			Apparent time.			
			h	′	″	
1761.	June	23.	15	10	24	Immerſion of the third ſatellite.
	July	20.	15	54	28	- - - of the ſecond.
		22.	12	35	29	- - - of the firſt.
	Aug.	7.	10	51	52	- - - of the firſt.
	Sept.	8.	10	28	5	- - - of the ſecond.
		10.	11	27	6	- - - of the third.
		15.	13	6	36	- - - of the ſecond.
		24.	8	7	46	Emerſion of the firſt.

Eclipſes of Jupiter's ſatellites, obſerved at Mr. Short's houſe in Surry-ſtreet in the Strand, London, by Dr. Bevis, with a reflecting teleſcope of four feet focal length, magnifying 140 times, and by Mr. Short, with a reflector of two feet focus, magnifying 95 times.

			Apparent time.				
			h	′	″		
1761.	July	22.	12	35	13	Immerſion of the firſt ſatellite,	by Dr. Bevis.
			12	34	58	- - - - - - - - - - -	by Mr. Short.
	Aug.	7.	10	29	43	- - - of the 2d ſatellite,	by Dr. Bevis.
			10	29	31	- - - - - - - - - -	by Mr. Short.
		27.	8	41	16	- - - of the 4th ſatellite,	by Dr. Bevis.
			8	37	4	- - - - - - - - - -	by Mr. Short.
		30.	11	7	31	- - - of the firſt ſatellite,	by Mr. Short.
	Sept.	8.	10	29	3	- - - of the 2d ſatellite,	by Dr. Bevis.
			10	28	35	- - - - - - - - - -	by Mr. Short.

N. B. Mr. Short's houſe is 26″½ of time to the weſt of the Royal Obſervatory.

LXI. *Lati-*

LXI. *Latitude of the Obſervatory at the Cape of* Good Hope, *reduced from the Obſervations of different Stars*; *by Mr.* Charles Maſon.

Read June 17, 1762.

1761.	Lat. South.	
	° ′ ″	
June 19. 23.	33 55 41½	By Antares.
June 21. 22. 23.	33 55 40½	Per Spica.
June 22. 23.	55 45½	Arcturus.
July 2. 3.	55 31	Fomalhaut.
9.	55 37	
Sept. 22.	55 43	
July 2. 3.	55 30	μ Scorpii.
July 1. 2. 3.	55 47	λ Scorpii.
July 2. 3.	55 44	δ Sagittarii.
July 12. Aug. 8.	55 29	σ Scorpii.
Aug. 8. 10.	55 40	Antares.
Sept. 22.	51 57	α Aquilæ.
Mean =	33 55 40½	Or, by leaving out the obſervation of
α Aquilæ, and three of the leaſt, it gives -	33 55 42 +	= Latitude South.

Charles Maſon.

LXII. *An Account of an Obſervation of the Tranſit of* Venus *over the Sun, on the 6th of* June 1761, *at* Madraſs; *by the Rev. Mr.* William Hirſt, *Chaplain of one of his Majeſty's Ships in the* Eaſt Indies: *Contained in a Letter wrote by him to the Right Honourable the Earl of* Macclesfield, *Preſident of the Royal Society. Dated Fort* St. George, 1*ſt* July 1761.

Read April 22, 1762.

MR. Hirſt began to make obſervations for regulating his clock, near three weeks before the day of the tranſit of Venus, by taking equal altitudes firſt, and then by meridional paſſages of Spica virginis, and of the Sun; of which latter, he had a good obſervation on the day before the tranſit, and another good one the day after it; ſo that there can be no doubt as to the accuracy of his time.

The place of his obſervation was fort St. George, on the top of the governor's houſe, whoſe latitude, as determined by many obſervations made not long ago, with an excellent quadrant, Mr. Hirſt ſays, is 13° 8′ N. and he makes it 3 minutes and 4 ſeconds of time eaſtward of Pondichery.

Mr. Hirſt's clock was made by M. Gallonde of Paris, and was conſtructed for aſtronomical uſes; it did not ſtop in winding up, and ſcaped dead ſeconds.

The teleſcope Mr. Hirſt obſerved with, was a reflecter 2 feet long, made by Mr. Adams, of Fleet-ſtreet, London, and lately ſent, as a preſent, by the Eaſt India company, to the Nabob Mahommed Allah Cawn, of whom Governor Pigot was ſo kind to borrow it, on this occaſion. The governor himſelf, and alſo Mr. Call, a very ingenious gentleman, aſſiſted in the obſervation; the former with a 4 feet refracter, of Mr. Dollond's new conſtruction; the latter with a 2 feet reflecter, formerly belonging to Dr. Mead.

Some time before five, in the morning of the 6th of June, Mr. Hirſt, and the reſt of the gentlemen, met on the terraſs of the fort-houſe, and were at their glaſſes, at the time the Sun roſe, leſt Venus might enter the diſk before the time calculated by the aſtronomers. The Jeſuits had calculated the beginning for Pondichery, at $6^h\ 57'$. The London calculations, reduced to the meridian of fort St. George, gave it at $7^h\ 26'\ 35''$ apparent time.

The morning proved favourable to the utmoſt of their wiſhes, which the more increaſed their impatience. At length, as Mr. Hirſt was ſtedfaſtly looking at the under limb of the Sun, towards the ſouth, where he expected the planet would enter, he plainly perceived a kind of penumbra, or duſky ſhade; on which he cried out, *'tis a-coming*, and begged Mr. Call to take notice of it. Two or three ſeconds after this, namely, at $7^h\ 31'\ 10''$ apparent time, happened the firſt exterior contact of Venus with the Sun, which all the three obſervers pronounced at the ſame inſtant, as with one voice. Mr. Hirſt is apprehenſive, that to be able to diſcern an atmoſphere about a planet at ſo great a diſtance as Venus, may be regarded as chimerical; yet affirms, that ſuch nebuloſity was ſeen by them, without preſuming to aſſign the cauſe. They loſt ſight of this phænomenon as the planet entered the diſk, nor could Mr. Hirſt perceive it after the egreſs.

The total ingreſs, or firſt internal contact, was determined with a preciſion equal to that of the firſt external contact, at $7^h\ 47'\ 55''$ apparent time.

Mr. Hirſt thinks it neceſſary to take notice of another odd phænomenon. At the total immerſion, the planet, inſtead of appearing truly circular, reſembled more the form of a bergamot pear, or, as Governor Pigot then expreſſed it, looked like a ninepin; yet the preceding limb of Venus was extremely well defined. Mr. Hirſt ſuſpected this appearance might be owing to their teleſcopes not being nicely enough ſet to their focal lengths: accordingly, he took care to try this ſeveral times, during the tranſit, but found it not to be the caſe; for though the planet was as black as ink, and the whole body truely circular, juſt before the beginning of the egreſs, yet it was no ſooner in contact with the Sun's preceding limb, than it aſſumed the ſame figure as before, at the Sun's ſubſequent limb; the ſubſequent limb of Venus keeping well defined, and truely circular.

The beginning of the egreſs, or ſecond interior contact, was obſerved only by Mr. Hirſt and Mr. Call, Mr. Pigot having retired. This phaſis came on at $1^h\ 39'\ 38''$, P. M. and the total egreſs, by Mr. Hirſt alone, at $1^h\ 55'\ 44''$, apparent time, Mr. Call unfortunately loſing the ſolar image out of the field of his teleſcope.

LXIII. *An*

LXIII. *An Account of a printed Memoir, in* Latin, *preſented to the Royal Society, intituled,* De Veneris ac Solis congreſſu obſervatio, habita in aſtronomicâ ſpeculâ Bononienſis Scientiarum Inſtituti, die 5 Junii 1761. Auctore Euſtachio Zanotto, ejuſdem Inſtituti Aſtronomo, ac Regiæ utriuſque Londinenſis et Berolinenſis Academiæ Socio. *By* Nathanael Bliſs, Savilian *Profeſſor of Geometry, and F. R. S.*

Read July 1, 1762.

THE planet Venus hath been ſo ſeldom obſerved in thoſe circumſtances, which are of the greateſt uſe in determining ſome of the moſt eſſential elements of its motion, that every ſuch obſervation, made by an accurate aſtronomer, cannot but be very acceptable to the public.

At Bologna, on the night preceding the day of the tranſit, the weather was very unfavourable; but early in the morning, the clouds, which covered the whole hemiſphere, began to break, and were driven off towards the horizon, by a gentle wind: ſo that the obſervations were retarded only during the ſpace of about half an hour. Father Friſi, profeſſor of mathematics at Piſa, and Signors Mathenci and Marini, aſſiſted in making the obſervations; the two latter obſerving, in the upper room of the obſervatory, together with Mr. Profeſſor Zanotti; and Father Friſi, accompanied by the two profeſſors of mathematics Signors Caſali and Canterzani, in a lower chamber.

S. Zanotti, in order to determine the place of Venus on the Sun, made uſe of a quadrant of 2½ feet radius, in the teleſcope of which were placed two wires, the one in an horizontal, the other in a vertical direction: by obſerving the appulſes of the limbs of the Sun and Venus to theſe wires, ſucceſſively, no error from refraction can take place. But it is of no ſmall conſequence to the accuracy of theſe obſervations, that the wires ſhould be placed truly perpendicular to each other. For this purpoſe, the quadrant was placed in the plane of the meridian, and a ſtar, during its tranſit, was obſerved more than once, accurately to run along the horizontal wire. Though the poſition of the vertical wire was often tried by terreſtrial objects, yet other methods of examination were made uſe of. At the ſame altitude, both before and after noon, the paſſage of the Sun not only over the horizontal, but alſo over the vertical wire, was obſerved, that it might from thence appear, whether the times of paſſage, when the neceſſary errors in obſerving are allowed for, were equal in both caſes. In each of the following obſervations, the altitude is not nicely determined; becauſe an error of one degree would occaſion little or no difference in the quantity of the parallax.

The obſervations, fourteen in number, as given by the author, follow:

Obſervation 1ſt. Altitude 5° 14′.

H	′	″	
16	54	37	⊙'s preceding limb at the horizontal wire.
	54	45½	⊙'s preceding limb at the vertical wire.
	56	15	♀'s preceding limb at the vertical wire.

H	′	″	
16	56	20	♀'s conſequent limb at the vertical wire.
	57	20	♀'s preceding limb at the horizontal wire.
	57	26	♀'s conſequent limb at the horizontal wire.
	57	54	☉'s conſequent limb at the horizontal wire.
16	57	55	☉'s conſequent limb at the vertical wire.

Obſervation 2d. Altitude 7° 0′.

H	′	″	
17	5	56½	☉'s preceding limb at the horizontal wire.
	5	59½	☉'s preceding limb at the vertical wire.
	7	25½	♀'s preceding limb at the vertical wire.
	7	30½	♀'s conſequent limb at the vertical wire.
	8	35½	♀'s preceding limb at the horizontal wire.
	8	40½	♀'s conſequent limb at the horizontal wire.
	9	11½	☉'s conſequent limb at the horizontal wire.
17	9	13	☉'s conſequent limb at the vertical wire.

Obſervation 3d. Altitude 8° 10′.

H	′	″	
17	12	50½	☉'s preceding limb at the horizontal wire.
	12	53	☉'s preceding limb at the vertical wire.
	14	16	♀'s limb at the vertical wire.
	14	22	♀'s conſequent limb at the vertical wire.
	15	27	♀'s preceding limb at the horizontal wire.
	15	32	♀'s conſequent limb at the horizontal wire.
	16	4	☉'s conſequent limb at the horizontal wire.
17	16	7	☉'s conſequent limb at the vertical wire.

Obſervation 4th. Altitude 9° 8′.

H	′	″	
17	19	24	☉'s preceding limb at the horizontal wire.
	19	29	☉'s preceding limb at the vertical wire.

H	′	″	
17	20	50	♀'s preceding limb at the vertical wire.
	20	55½	♀'s conſequent limb at the vertical wire.
	21	57½	♀'s preceding limb at the horizontal wire.
	22	3	♀'s conſequent limb at the horizontal wire.
	22	35	☉'s conſequent limb at the horizontal wire.
17	22	45½	☉'s conſequent limb at the vertical wire.

Obſervation 5th. Altitude 10° 50′.

H	′	″	
17	29	41	☉'s preceding limb at the horizontal wire.
	29	55½	☉'s preceding limb at the vertical wire.
	31	14½	♀'s preceding limb at the vertical wire.
	31	20	♀'s conſequent limb at the vertical wire.
	32	10	♀'s preceding limb at the horizontal wire.
	32	16	♀'s conſequent limb at the horizontal wire.
	32	50½	☉'s conſequent limb at the horizontal wire.
17	33	15½	☉'s conſequent limb at the vertical wire.

Obſervation 6th. Altitude 14° 12′.

H	′	″	
17	49	38½	☉'s preceding limb at the horizontal wire.
	49	42½	☉'s preceding limb at the vertical wire.
	50	55	♀'s preceding limb at the vertical wire.
	51	1½	♀'s conſequent limb at the vertical wire.
	51	58½	♀'s preceding limb at the horizontal wire.
	52	4½	♀'s conſequent limb at the horizontal wire.
	52	42½	☉'s conſequent limb at the horizontal wire.
17	53	7½	☉'s conſequent limb at the vertical wire.

Obſervation 7th. Altitude 17° 0′.

H	′	″	
18	6	3½	☉'s preceding limb at the horizontal wire.
	6	15	☉'s preceding limb at the vertical wire.
	7	11	♀'s preceding limb at the vertical wire.
	7	17	♀'s conſequent limb at the vertical wire.
	8	31	♀'s preceding limb at the horizontal wire.
	8	36½	♀'s conſequent limb at the horizontal wire.
	9	18	☉'s conſequent limb at the horizontal wire.
18	9	31½	☉'s conſequent limb at the vertical wire.

Obſervation 8th. Altitude 23° 40′.

H	′	″	
18	44	36½	☉'s preceding limb at the horizontal wire.
	45	15½	☉'s preceding limb at the vertical wire.
	46	7½	♀'s preceding limb at the vertical wire.
	46	14	♀'s conſequent limb at the vertical wire.
	46	39½	♀'s preceding limb at the horizontal wire.
	46	47	♀'s conſequent limb at the horizontal wire.
	47	36	☉'s conſequent limb at the horizontal wire.
18	48	49	☉'s conſequent limb at the vertical wire.

Obſervation 9th. Altitude 31° 42′.

H	′	″	
19	30	15	☉'s preceding limb at the horizontal wire.
	30	22	☉'s preceding limb at the vertical wire.
	30	59	♀'s preceding limb at the vertical wire.
	31	5	♀'s conſequent limb at the vertical wire.
	32	6	♀'s preceding limb at the horizontal wire.
	32	11	♀'s conſequent limb at the horizontal wire.
	33	11½	☉'s conſequent limb at the horizontal wire.
19	34	0	☉'s conſequent limb at the vertical wire.

Obſervation 10th. Altitude 34° 15′.

H	′	″	
19	44	10½	☉'s preceding limb at the horizontal wire.
	44	26½	☉'s preceding limb at the vertical wire.
	44	58½	♀'s preceding limb at the vertical wire.
	45	5½	♀'s conſequent limb at the vertical wire.
	45	59	♀'s preceding limb at the horizontal wire.
	46	4½	♀'s conſequent limb at the horizontal wire.
	47	7½	☉'s conſequent limb at the horizontal wire.
19	48	4	☉'s conſequent limb at the vertical wire.

Obſervation 11th. Altitude 37° 21′.

H	′	″	
20	2	1½	☉'s preceding limb at the horizontal wire.
	2	14	☉'s conſequent limb at the vertical wire.
	2	38	♀'s preceding limb at the vertical wire.
	2	44	♀'s conſequent limb at the vertical wire.
	3	46½	♀'s preceding limb at the horizontal wire.
	3	52	♀'s conſequent limb at the horizontal wire.
	4	59½	☉'s conſequent limb at the horizontal wire.
20	5	49	☉'s conſequent limb at the vertical wire.

Obſervation 12th. Altitude 41° 7′.

H	′	″	
20	23	0½	☉'s preceding limb at the horizontal wire.
	23	1½	☉'s preceding limb at the vertical wire.
	23	18	♀'s preceding limb at the vertical wire.
	23	24½	♀'s conſequent limb at the vertical wire.
	24	41½	♀'s preceding limb at the horizontal wire.
	24	48	♀'s conſequent limb at the horizontal wire.
	26	0	☉'s conſequent limb at the horizontal wire.
20	26	36	☉'s conſequent limb at the vertical wire.

Obſervation 13th. Altitude 44° 10′.

H	′	″	
20	40	16	☉'s preceding limb at the horizontal wire.
	40	22	☉'s preceding limb at the vertical wire.
	40	33½	♀'s preceding limb at the vertical wire.
	40	39	♀'s conſequent limb at the vertical wire.
	41	56½	♀'s preceding limb at the horizontal wire.
	42	1½	♀'s conſequent limb at the horizontal wire.
	43	17½	☉'s conſequent limb at the horizontal wire.
20	43	53½	☉'s conſequent limb at the vertical wire.

Obſervation 14th. Altitude 46° 28′.

H	′	″	
20	53	51½	☉'s preceding limb at the horizontal wire.
	53	58½	☉'s conſequent limb at the vertical wire.
	54	3½	♀'s preceding limb at the vertical wire.
	54	9	♀'s conſequent limb at the vertical wire.
	55	30	♀'s preceding limb at the horizontal wire.
	55	36	♀'s conſequent limb at the horizontal wire.
	56	54	☉'s conſequent limb at the horizontal wire.
20	57	25½	☉'s conſequent limb at the vertical wire.

When the planet drew near to the edge of the Sun's diſk, the obſervers prepared to determine the time of the two contacts, Profeſſor Zanotti, with the teleſcope of the quadrant of 2½ feet focus, Profeſſor Mathenci, with the teleſcope of 22 feet, and Signor Marini, with that of 10 feet.

The internal contact was obſerved

	H	′	″		
At	21	4	34	with the teleſcope of	2½ feet.
	21	4	58	- - - - - - -	10 feet.
	21	4	58	- - - - - - -	22 feet.

The external contact was obſerved

	H	′	″		
At	21	22	30	with the teleſcope of	2½ feet.
	21	23	0	- - - - - - -	10 feet.
	21	23	7	- - - - - - -	22 feet.

During the intervals of the obſervations made with the quadrant, the planet was always obſerved to be perfectly round, without any ring or nebuloſity.

It may, at firſt ſight, ſeem wonderful, ſays Signor Zanotti, that obſervations made with different teleſcopes, one of 10, the other of 22 feet, ſhould ſo nearly coincide, the times of the firſt contact agreeing to the ſame ſecond, and thoſe of the laſt differing only 7 ſeconds, by which the contact was ſeen to happen ſo much later through the longer teleſcope; and the blame might be laid either upon the longer teleſcope, or upon the obſerver. The goodneſs of the teleſcope will readily be allowed, when it is known, that it was made by Campani; and the ſkill and dexterity of the obſerver are too well known, to give room for any ſuſpicion on his part. It may rather be attributed to the near equality of the magnifying power of the two inſtruments; the longer teleſcope having an eye-glaſs of 3 inches focal length, and the ſhorter an eye-glaſs of 1¼; by means of which, the images of the Sun and Venus were nearly equal in both.

The author then proceeds to determine, by calculation, (the method of which he has at large explained) the difference of longitude between the centers of the Sun and Venus; and alſo the planets latitude, which, as ſeen from the Earth's center, are, at the time of each obſervation, as in the following table.

N. B. The author has not mentioned the exact quantity of the Sun's parallax, which he made uſe of in theſe computations: but, from ſome trials, it ſhould ſeem, that he ſuppoſed the parallax of the Sun to be $10\frac{1}{2}$ or 11 ſeconds.

True time, after the noon.	Difference of longitude between ⊙ and ♀.	Latitude ♀ South.
H ′ ″	′ ″	′ ″
16 56 17½	5 46 Eaſt.	8 31
17 7 28	5 7 Eaſt.	8 40½
17 14 19	4 41½ Eaſt.	8 46
17 20 52½	4 15½ Eaſt.	8 56
17 31 17½	3 36½ Eaſt.	8 54
17 50 58½	2 18 Eaſt.	9 0
18 7 14	1 21½ Eaſt.	9 14
18 46 10½	1 19 Weſt.	9 46
19 31 12	4 19½ Weſt.	10 4
19 45 2	5 1½ Weſt.	10 13
20 2 41	6 29½ Weſt.	10 28½
20 23 21	7 46½ Weſt.	10 41
20 40 36½	8 46 Weſt.	10 49
20 54 6½	9 46 Weſt.	11 0

Theſe longitudes and latitudes do not exactly anſwer to the interval of time between each obſervation: but the obſerver has related them faithfully as they were taken; and if we conſider, that they were determined by time, and that an error of half a ſecond will have a conſiderable influence upon each obſervation, it will readily be allowed, that the obſervations are carefully made, and agree very well together, though taken with an inſtrument of ſo ſmall a radius. The following are the elements deduced from thoſe obſervations, which were made at the diſtance of at leaſt an hour and an half:

	°	′	″
The horary motion of ♀ in longitude -	0	3	55$\frac{4}{5}$
The horary motion in latitude - - - -	0	0	36$\frac{3}{5}$
	H	′	″
The true time of the conjunction of ☉ and ♀. - - - - - - - - - - -	18	26	0
	°	′	″
The latitude of ♀ at the conjunction -	0	9	27$\frac{5}{6}$

From theſe numbers the author deduced the following elements, by trigonometrical calculation:

	°	′	″
The angle of the path with the ecliptic	8	49	23
The horary motion in the path - -	0	3	58$\frac{2}{3}$
The part of the path between the middle of the tranſit and the conjunction -	0	1	27
The diſtance of the path from ☉'s center ſouthwards - - - - - - - -	0	9	21
The length of the path within the ☉'s diſk - - - - - - - - - - -	0	25	29$\frac{1}{2}$
The difference of longitude of ☉ and ♀ at the ingreſs - - - - - - -	0	14	2
The difference of longitude at the egreſs	0	11	9$\frac{1}{2}$
The latitude of ♀ at the ingreſs ſouth -	0	7	17
The latitude of ♀ at the egreſs ſouth -	0	11	12
	H	′	″
The time of the middle of the tranſit -	18	4	6
The ingreſs of the center of ♀ on the ☉'s diſk - - - - - - - - - - -	14	51	49
The egreſs of the center of ♀ - - - -	21	16	23

It

It appears alſo by his calculation, that the time of the internal contact was accelerated 30″, and the laſt contact 18″; by parallax. The internal contact, therefore, as ſeen from the center of the Earth, was at 21^h 5′ 28″, and the external contact was at 21^h 23′ 25″, and the egreſs of the planet's center at 21^h 14′ 33″.

From the time of the planet's paſſage over the edge of the Sun's diſk, as ſeen from the Earth's center, the author very accurately determines the planet's diameter to be $57''\frac{2}{3}$.

The egreſs of the center of Venus, as deduced from the poſition of its path, and from the other elements, as related above, differs near two minutes from the obſerved time, when corrected by parallax, and reduced to the Earth's center. This difference is entirely to be attributed to an error in the motion of Venus in longitude, which, perhaps, could not be deduced with ſufficient accuracy from theſe obſervations, and from a ſmall error in ſome of the other elements; all which the author might have taken, with the utmoſt accuracy, from the tables either of Dr. Halley or M. Caſſini. Perhaps alſo, ſome part of this difference might ariſe from our ignorance of the true quantity of the Sun's parallax.

Hitherto our author has given us thoſe elements, which might immediately be determined from his obſervations: the following are deduced from the tables. From the the motion of Venus in latitude, it may readily be collected, that the planet was in its node on June 5, at 14^h 55′ 9″. The place of the Sun at that time, according to the tables of the Abbé De la Caille, was in ♊ 14° 59′ $5''\frac{1}{2}$; and the planet's elongation from the Sun, at the ſame time, was 1° 0° 58″. Therefore, the longitude of Venus; and alſo of the node, was in ♊ 13° 58′ 7″½. The angle at the Sun, or the difference of the longitude of the planet and the Earth, as ſeen from the Sun, was 0° 24′ 15″. Therefore, the longitude of the deſcending node of Venus, as ſeen from the Sun, was in ♐ 14° 34′ 50″.

The latitude of Venus, as ſeen from the Earth, at the time of the conjunction, was 0° 9′ 27″⅙; by ſolving a triangle of which, the computed diſtances of the Earth and Venus from the Sun conſtitute two ſides, the angle at the Sun, or the planet's heliocentric latitude, viz. 0° 3′ 46″, will be determined. With this heliocentric latitude, and the calculated place of the Sun at the time of the conjunction, and the longitude of the node, as before laid down, from two ſides of a ſpheric right-angled triangle, an angle may be computed, which will expreſs the inclination of the planet's orbit with the ecliptic. The place of the Sun, at the time of the conjunction, was in ♊ 15° 36′ 10″. The difference of the heliocentric longitude of the earth, and the node, was 1° 1′ 20″. Therefore the angle of the inclination of the orbit of Venus with the ecliptic is 3° 30′ 49″.

N. B. The ſeveral numbers contained in this paper, are taken from the correct numbers written in the margin of the printed memoir, with the author's own hand, and which ſeem to be the reſult of his lateſt calculations. And though his obſervations were made with great care, and faithfully calculated, yet the reſults will not be found ſo accurate, as could be wiſhed; ſince the latitude of Venus, deduced from theſe obſervations, is, in all probability, 10″ or 12″ too little; a quantity, which muſt have a very ſenſible influence, both on the place of the node, and the inclination of the planet's orbit with the ecliptic; the latter of which ought to be deduced from obſervations made on the planet, when in its greateſt latitudes.

In the lower chamber of the obſervatory, the obſervers made uſe of two teleſcopes, one of 6, the other of 8 feet, furniſhed with wires at half-right angles, in order to determine the place of Venus on the Sun, by cauſing the Sun's ſouthern limb to run down one of the threads: the following obſervations were made:

Obſervation 1ſt.

H	′	″	
18	11	40½	Sun's center at the horary wire.
18	11	50	Venus's center at the horary wire.
		26	The difference between the horary and oblique wires.

Obſervation 2d.

H	′	″	
19	24	1½	Center of ♀ at horary wire.
19	24	17½	Center of ⊙ at horary wire.
		23	Difference between the horary and oblique wires.

Obſervation

Obſervation 3d.

H	′	″	
20	16	53	Center of ♀ at horary wire.
20	17	23	Center of ⊙ at horary wire.
		20	Difference between the horary and oblique wires.

Obſervation 4th.

H	′	″	
20	47	22½	Center of ♀ at horary wire.
20	47	55½	Center of ⊙ at horary wire.
		17	Difference between the horary and oblique wires.

Obſervation 5th.

H	′	″	
20	59	17	Center of ♀ at horary wire.
20	59	54½	Center of ⊙ at horary wire.
		15¾	Difference between the horary and oblique wires.

The internal contact was obſerved, by three different teleſcopes,

	H	′	″		
At	21	4	54	with a teleſcope of	6 feet.
	21	5	0	- - - - - - - -	8 feet.
	21	4	56	- - - - - - - -	11 feet.

The external contact was obſerved

	H	′	″		
At	21	22	53	with a teleſcope of	6 feet.
	21	22	50	- - - - - -	8 feet.
	21	22	59	- - - - - -	11 feet.

Profeſſor Canterzani examined the obſervations by projection, and found them to agree very nearly with thoſe made in the upper chamber by Signor Zanotti.

END of PART I.

ERRATUM.

Page 198, Line 11, for *from,* read *with.*

ERRATUM in VOL. LI. PART II.

Page 922, Line 2, for *ſum or difference,* read *difference or ſum.*